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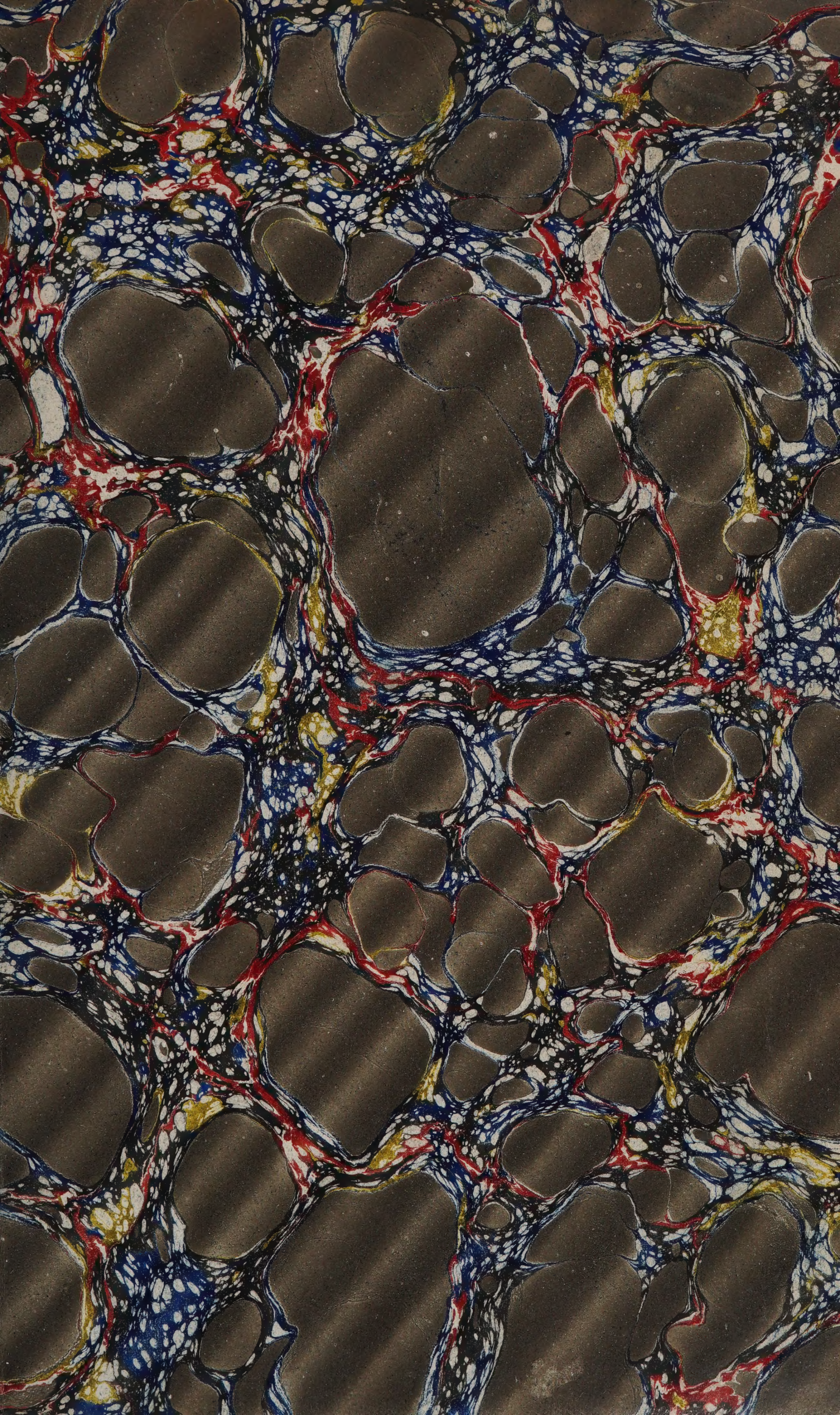

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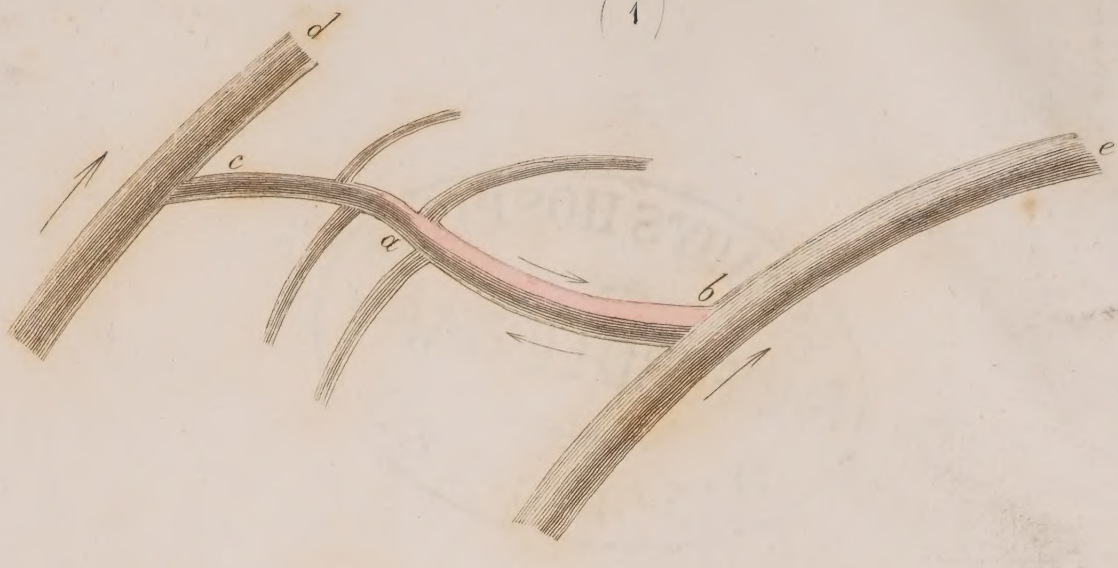

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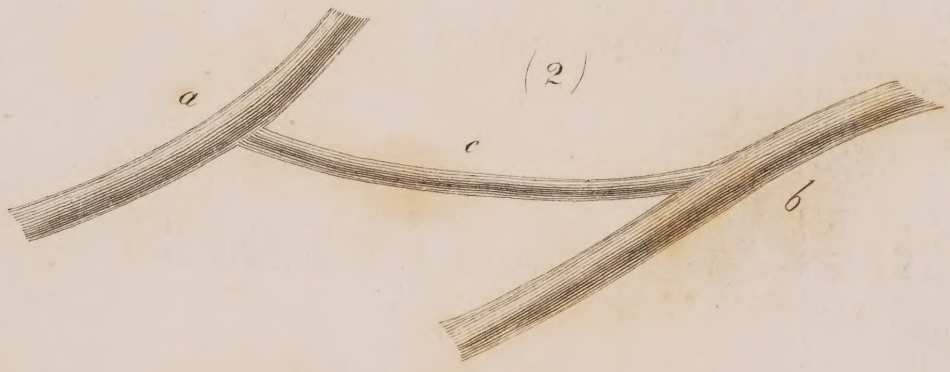

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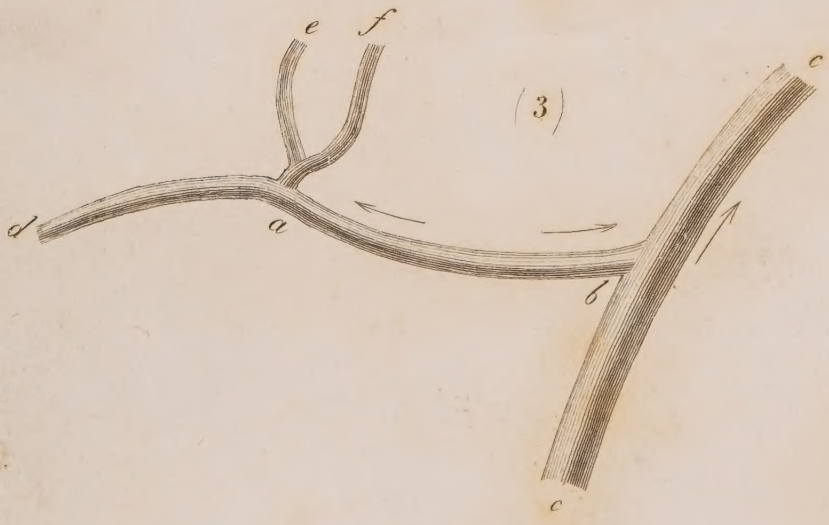

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INTRODUCTORY ESSAYS,

BY

CHARLES HENRY PARRY, M.D. F.R.S.

And Member of many other Societies, British and Foreign.

"THROUGH EVIL REPORT, AND THROUGH GOOD REPORT."

1825.

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PREFACE.

TEN years have nearly elapsed since the publication of the "Elements of Pathology and "Therapeutics," and nearly three years since the release of its highly valued Author from a long and complicated state of suffering and disease. In the year previous to his lamented illness, he had completed this first volume of a work, which was intended to contain "an Epitome of his Pathological and "Therapeutical Principles," or a very general view of the objects, which, as well with regard to the character as the treatment of diseases, appeared to him the most obvious, and, therefore, the most deserving attention. This abstract was prepared at the repeated solicitation of some of his friends, who were aware of the demand made upon his time by numerous other avocations, and who had, with anxiety, witnessed several attacks of acute disease with which he had lately been afflicted. They were likewise apprehensive, that, in delaying the publication of his

opinions, under the hope of extending materials, which were already very considerable, and were daily accumulating in his hands, a final arrangement might either be entirely prevented; or, that the peculiarities, at least, which had been gradually adopted by many of those with whom the Author was in the habit of frequent intercourse and communication, might, at a subsequent period, cease to be redeemable as his own property. It had, indeed, been a matter of regret to those who knew, and estimated the merits of Dr. PARRY, that the daily publications abounded with facts, and observations, which were recorded as important novelties; which had, nevertheless, long been familiar to him, and had then, as himself expresses it, "for thirty years formed the "basis of his practice." In this situation was he placed with regard to many nervous disorders, and particularly with regard to the frequent inflammatory nature of Dropsy, a subject which among others, from materials privately furnished by himself, had supplied the numbers of certain periodical works with Essays, to which other names were attached, and which had much contributed to the reputation of their respective authors.

At the solicitation of his friends, Dr. Parry prepared his first volume. He was proceeding to a second, which was to contain his therapeutical views, when visited by that malady, which, for a long time, impaired his mental faculties, and, for a series of melancholy years, deprived him not only of the means of communicating his ideas by writing, but also

interrupted his power of speech. In his most vigorous health he had never possessed those facilities, which, to most writers, have appeared necessary for the proper arrangement of the materials which may have been collected in the hurry of the world's affairs. He was totally unacquainted with that comparative leisure and seclusion, which have, by some philosophers, been supposed no less essential to composition, than, in the poet's estimate, to an intimate knowledge of ourselves, and a preparation for a more perfect state of being.

> "If in society we learn to live,
> "'Tis solitude must teach us how to die!"

Under the disadvantages arising from various and continued avocations, Dr. Parry was commencing a second volume, when he was finally interrupted in the prosecution of his design, and debarred from the further use of the various stores which he had either treasured up in his memory, or had secured by writing, for future reference and application.

While, however, as he has himself expressed it, neither his "Elements," nor the work of which it was the outline, was intended as a complete system of animal pathology, or of all the diseases to which the frame is liable; he reserved the recital of the very numerous cases and dissections, which are proofs of the principles there introduced, for a much larger work, which he had long meditated, and which he still hoped that he might one day be able to accomplish. He proceeds to say, that "if it shall please "Providence to frustrate that hope, the Author has

"great satisfaction in leaving his materials in the "hands of a person, deservedly most dear to him, "of whom it may be permitted him to assert, that "his talents, as well as general and professional "knowledge, render him fully competent to the "execution of that task."* If, however, in his calculations as to the remaining period of his mental and physical capacity, and in his hope of leaving his materials in a sufficiently forward and arranged state for public inspection, the Author's wishes and expectations were frustrated by the interdicting hand of Providence; he was still more incorrect and fallible in his estimate of the abilities and power of that person, to whose care his abundant and valuable, but heterogeneous and unconnected, materials were ultimately consigned. To this person, it may be said, in truth, the desire of perpetuating, in his important doctrines, the memory of a most beloved and much admired parent, would have been an animating passion, and a sufficient stimulus, had this sacred call never been made upon his time and upon his exertions. To him it would ever have been a matter of solicitude, that those opinions should be rightly understood; that they should be maintained by his best, however weak, endeavours; and that they should be confirmed and illustrated by all the authority and evidence, which could be brought to their support. This imperative and affectionate appeal has, indeed, rendered doubly difficult that task, which would, at all times, have required his whole devotion. In con-

* Preface, p. iv.

sequence of the expectation which has been raised, and by which he has become, in some measure, committed with the public, much hesitation and distrust have unavoidably mingled themselves with his wishes and intentions. These feelings have arisen, as well from a sense of his own incapacity, and the manifest difficulty in determining the best mode of securing, if not adding to, the Author's well-earned reputation, as from an anxiety to accomplish his object in a manner which should not be unworthy of a liberal and enlightened public. In looking at the materials which have been left to his care and disposal, he finds himself in the situation of one who is suddenly placed amid the half-finished specimens, the friezes, capitals, and embryo columns of some master architect, which, united by his hand and under his guiding intellect, would hereafter have grown into perfect forms, and become arranged into some noble structure, sacred and devoted to his own perpetual fame. In the absence of this directing genius, they remain either half-shapen masses, indicative of beauty and energy; or, set together in combinations foreign to the original design, while their separate beauty ceases to be conspicuous, they fail also to impart strength and dignity to the elevation, of which they now constitute an incongruous part.

In admitting that the public has a claim upon these relics, and in preferring, on many accounts, an unconnected and disjointed form, to the attempt at a consolidation, which will either not resemble, or will fall short of, the original design, the Editor is con-

scious that some further apologies may appear to be due, on the ground of a presumed procrastination, and of unnecessary delay. Besides the reasons which have been already specified, it is, however, of little import to the public, to be made acquainted with the various, unavoidable, and domestic causes which have hitherto stood in the way of an attention, which he has ever been most anxiously disposed to bestow upon these subjects. To himself it is a sufficient justification, that his friends are ready to vindicate him from any imputation of voluntary neglect; and it may be sufficient to assure the public, that no loss has been sustained in consequence of the alleged delay. He states it, on the contrary, as his firm conviction, that the period which has intervened between the publication of the "Elements" and the present time, has been most profitably occupied by the full consideration, which has thus been allowed, of the general and particular principles developed in this, and in the Author's other works; and by the discussion, which has thus been excited amongst men of the first talents, as well in reference to these peculiar doctrines, as to other connected subjects, which have been, by this means, exposed to more open light, or submitted to a more exact and rigorous enquiry. In the space of nearly ten years the merits of Dr. Parry's chief work have been most strictly canvassed. Every thing has, probably, now been said, which can, in fairness, be urged against it. It may, perhaps, be stated with truth, that a delay in the re-publication of that volume, and in the communication of the

materials which were devoted to its continuation, and to that of the much more extended work proposed by the Author, has been advantageous, if it has enabled us to peruse and examine the profound and elaborate commentaries of Dr. Pring alone, on many of these important subjects. By having had leisure to consider the force of various objections, the public is, in fact, better prepared for the reception of Dr. Parry's opinions, whether they appear in an old or in a new form. They will be more disposed to hail the arrival of an old acquaintance, which has stood the test of much criticism, and be anxious to examine whether, and by what new arguments, facts, or illustrations, the objections advanced by others against particular doctrines, may be removed or sustained.

Much, indeed, has been written on the subject of Dr. Parry's doctrines, and it would manifest a strange defect of proper feeling and grateful sentiment, if the Editor did not acknowledge that, even where dissent has been proclaimed, the consideration of the subject has been marked with a peculiar degree of kindness, liberality, and candour. To the periodical Reviewers, who, under shelter of anonymous criticism, have the power and privilege of employing strong expressions of disapprobation, and who, in their sweeping and general declamation, have too frequently the temptation to substitute assumption for proof, he cannot but express his sense of their indulgence; and, though in a few instances, which it may become necessary to specify, some of the common faults are discoverable which

arise from this mode of investigating truth; though, upon some occasions, a singular perversion and misconception of the true meaning has crept into their pages, though an acquaintance with the subject has not always characterised their remarks; and though observations have been hazarded, at the expense of a rigorous attention to logical accuracy, and even to an exact consistency; there is, on the whole, an evident disposition to be pleased. Objections are made with good humour, and with a frequent acknowledgement of obligation; a marked deference is paid to the zeal and talent of the Author, and there is a universal recommendation of the work to the careful perusal of all those who are interested in the most important views, practical as well as speculative, of the present day. While, however, a general sense has been admitted, of the politeness and feeling which distinguish the several critiques on the writings of Dr. Parry, it would not be easy to avoid a particular notice of the Medical and Chirurgical Journal; not only on account of the spontaneous honours which it is disposed, on all occasions, to pay this Author, but also, on account of a far greater intelligence, and acquaintance with his opinions, than are manifested by any other writer on these subjects.

A consideration of some of the numerous objections which have been made to the doctrines in question, becomes, in a great measure, the duty of an Editor, upon whom, especially, is imposed the additional task of submitting to the public a large mass of new matter, in general confirmatory and illustrative of

the same doctrines and sentiments. The admission of such errors as may seem important, and to have become perceptible under the improvements of time, and the progress of experience, will, it is hoped, give additional brilliancy and force to those facts or opinions which remain entire, and without such implication. While, on the other hand, the removal of those objections which may seem futile or irrelevant, will resemble the clearing away of the rubbish which has collected by time round the fragments of taste and of skill, and which might equally corrupt, by adhering to the new, and as yet unsullied, forms.

In adverting, briefly, to some of the objections which have been advanced, it is proposed to separate them into two principal divisions, including under the first such as are of a general nature, and under the second those which have a more specific and particular allusion. Among the first, may be placed those circumstances connected with the subjects of his enquiries, which Dr. Parry is stated to have altogether neglected and omitted: such are his supposed disregard of the nervous, the glandular, and absorbent systems; of the venous apparatus of the brain; of the quality of the blood; the objections to the order and arrangement of his matter, and his frequent repetitions; his supposed error in not comprehending all his principles in a single work; his too great fondness for generalization and system; the extreme simplicity of his doctrines; his adoption of a Universal Cause, and a common origin of diseases; his reasoning upon slight analogies, and insufficient

data; his adoption of many points, which are too hypothetical, or have not the merit of novelty; his admission of the same causes in complaints differing very much in their nature, &c. Under the second head may be placed a numerous list of objections derived from his particular doctrines of Inflammation, of Tonicity, of the want of an independent arterial power; from his opinion that inflammation increases the secretions, his praise of Gall and Spurzheim, his denial of diminished absorption as a cause of Dropsy; his conjectures as to the possible nature of Scurvy; his omission of certain sympathetic cases of Epilepsy, and of Hæmorrhage from the Lungs and Uterus, &c.

In opposition to his views, it is maintained by divers critics, that Mania is not invariably owing to increased momentum; that Nervous Diseases do not always arise from the brain, and are not occasioned by sanguineous impulse; that the general tendency of disease is not to prolong life; that Dropsy, for example, is a real disease; that full diet is not necessary to the production of Inflammation; that indulgence and plethora are not peculiar to the present times, to particular classes, or assignable as the supposed causes of disease; that spasmodic Asthma does not arise from affection of the mucous membrane of the Trachea alone; that there are better accounts of the Brain; that the distinction between Vertigo and Swimming of the Head is inaccurate, and leads to an injurious practice; that excessive momentum is not a cause of Scarlatina, and Measles; that there is not always effusion in Inflammation.

To each of these objections, and to many others of the same kind, a sufficient answer might perhaps be given, if it were a matter of real import to the reputation of the Author, to defend him against every possible difference of opinion, whether it originate in a correct, or in an erroneous, view of the matter under consideration. In a general way, it may be replied, that some of these objections are founded in a very obscure and imperfect conception of the doctrines they profess to criticize; that many of them are already sufficiently met by the Author himself; that others might have been spared, as more referrible to what has not been done, than to the particular questions which should have been entertained, and that, in a multitude of other cases, the question is less what is the exception under the Reviewer's particular opinion, than what was the general rule derived from the experience of Dr. Parry. It may, also, be remarked, that if, in many of the latter instances, this Author had not essentially differed from those who preceded him, or who had before treated the same subject, his labours might safely have been spared, as the toil or luxury of criticism would also have been superseded. To time, and the results of continued experience, may, without fear, be entrusted the determination of many practical and experimental points, on which such difference of opinion is still maintained.

With regard to the particular form in which Dr. Parry published his Elements, it will not be forgotten, that the inducement to prepare this "Epitome"

arose, as has been shewn, from the apprehension of his friends that "a single work," embracing his whole doctrines, and their proofs, might, perhaps, never appear; and from their opinion, that it was adviseable, to hasten the preparation of a shorter and more concentrated exposition of the principles, with which themselves were conversant, and which they judged to be of public interest and importance. It would, indeed, have been highly advantageous to the Author's reputation, had he been permitted to carry into execution his whole plan. Under the circumstances, we cannot, however, but rejoice in the volume, such as it is, which we now possess, although it had, in fact, no connection with the Author's own schemes, and was not comprehended under his original intentions.

In questioning the propriety of the order and arrangement which has been adopted in this volume, it is apprehended, that those who make the objection have not taken an enlarged view of the whole design, and of the intimate connection of the parts with one another. A tabular arrangement might, indeed, have assisted those who, by a natural association of ideas, could not connect the links of his plan. To himself, and others, this means has appeared superfluous and unnecessary. When viewed in its entirety, the present scheme is, in fact, a regular progression from the simplest facts of the circulating system, through the circumstances of its most decisive morbid affections, to a series of seemingly or really analogous cases, constituting the instances of simple or com-

plicated determinations of blood in particular seats of disease. The abstract, which is called the Elements, supplies, it is true, the mere titles of the matter, and a general notice of the conditions included under each division. In the particular, or Special Pathology, which was to follow, would have been comprehended, under a different arrangement, all the more definite and precise descriptions, as well as the more complex related qualities and forms of disease.

That, according to the present scheme, repetition was not only unavoidable, but by necessity adopted, is evident from the reasons assigned by the Author himself. "Examples," he says,* "have already been "adduced for the sake of the immediate illustration "of other principles, and will be repeated where they "may be found essential to the present view of the "subject." Again, "Various repetitions will have "been observed; but where it is designed to esta- "blish the order, as well as the quality, of phœno- "mena, such repetitions are absolutely unavoidable; "since the same fact or principle must often neces- "sarily form the nexus or connecting link of "different series."

On a whole view of the subject, instead of there being little order or arrangement, the parts appear to grow naturally out of one another, the alliances and affinities to be the nearest, and most readily suggested by previous trains of reasoning, and to connect, in the most natural manner, former opinions with succeeding

* Elements, pp. 299, 373.

facts and arguments. Independently of the necessity here suggested, repetitions may also, perhaps, be considered beneficial rather than impertinent, when, in an abstruse subject like the present, clearness is gained by their adoption.

It certainly is not very evident to those who have carefully perused this work, that "by neglecting the "ordinary mechanical divisions of sections and sub-"sections, the illustrations have often been needlessly "encumbered;"* and, we conceive the remark, that "such a want of regular division, and the division "into aphorisms of unequal lengths, and apparently "with little regard to order and arrangement, is "injudicious, inasmuch as it is very uninteresting to "the reader," to be not accordant with the feelings excited in the generality of readers, even by a first perusal. Upon such an occasion, and even with those whose associating power is not very strong, the present division into chapters, and the general titles, may serve instead of the more formal mode, and may spare the exertion of verbal memory, and even the more important and salutary association of ideas themselves. The present volume was, however, intended solely as an Abstract; and the aphorismal form was expressly adopted as conveying, in the least encumbered manner, propositions of reference, which were afterwards to expand into many sections and chapters.

If, however, Dr. Parry has met this particular objection himself, it may be still more decidedly maintained, that all his writings are a sufficient

* Ed. Med. and S. Journal, vol. xv. p. 94, 1819.

answer to a very large and common class of observations which are, in fact, founded on an ignorance of his doctrines, and which, with a sweeping and indiscriminate calculation, denounce against them the fate and consequences which have hitherto attended the adoption of universal causes and common origins of disease. To the Critics of this description may, with deference, be recommended the sober advice of Ben Johnson, upon a similar occasion.

> "Pray thee take care, that tak'st my book in hand,
> "To read it well: that is, to understand."*

After a cautious reading of the Elements of Pathology it can never, consistently with prudence, be objected to Dr. Parry, that he maintained "a common origin of all diseases, an all-pervading momentum, an universal cause, the derangement of one function or texture, or of one principle or agent to the exclusion of others."† Neither can it safely be argued, that the Author was under the dominion of a favourite theory, until the Critic has demonstrated in what particular examples, whether as regards pathological or therapeutical indications, the assumed concurrency and co-operation of one condition in the specified cases, is not founded on experience, and a tangible and important practical basis.‡

Of the spirit of generalization, and the excessive love of system, much has been said, which is not referrible to the doctrines before us. Till it is shewn

* Vol. iii. p. 117. Ed. 1716.

† Edin. Med. and Surg. Journal, No. xv. p. 93, &c.

‡ Lond. Med. Rep. vol. v. p. 115, (1816.)

that, under the arrangement which he has derived from his experience, Dr. Parry intended to include all diseases, and that the diseases so arranged are not connected by this common link, the objection spares the necessity for any further notice or consideration.

It has, indeed, been said by an anonymous Critic, in a Northern Review, that Dr. Parry's doctrines, as implying a Universal Cause, "cannot fare better than "the χωλη of Hippocrates, or the Pituita of Galen, "the fermentation of the Arabians, the viscidity and "lentor of the humoralists, or the lemmas of the "Mathematical Physicians." In an Author, to whom, on some accounts, gratitude and respect are due, the Editor does not willingly proclaim a complete unacquaintance with the subject he undertakes to criticize, and an entire absence of analogy in the instances which he advances, as illustrative of opinions, which he professes to understand in a better manner than his neighbours. Whatever may be the tendency of any doctrine towards an oblivion, from which the imperfection of human nature may possibly not be exempt, and admitting the error necessarily connected with the adoption of a Universal Cause, can it be maintained, that, because these unknown and undemonstrable existencies were insufficient as the foundations of a permanent system, the fluid universally pervading the animal body may not, therefore, be assumed as a concurrent cause in the greater number of maladies which affect the animal frame? Shall a tangible and visible substance, a material,

conveyed every where for the most important purposes of the animal economy, the source of nutrition, the supporter of life, the means of reparation, having a well-known and most extensive influence and connection with every organ and structure, be refused as the probable source and medium of many diseased actions? In its manifold relations, shall it be considered a circumstance analogous to the fanciful essences above enumerated? Or shall we even place it on a par with the φυσις of Archæus, the εμφαλον πυρ, the moving principle, the subtile and mobile matter, the nervous fluid, the vital principle, the electric and galvanic Aura of other authors? It may serve the purposes of display, to employ these and similar phrases; but, in candour, is the fact as stated? in knowledge, is the analogy supportable? Has not the Author of the Elements endeavoured to raise a standard in defence of experience and observation against many of these very absurdities? In its important and practical application, can the analogy be sustained, and will it be said that the doctrine which supposes the concurring implication of the sanguiferous system has no more value than, in their practical application, those to which it is, by this anonymous critic, opposed? Is the principle of effusion, and convulsion, as salutary effects, really analogous, in his eye, to the doctrine of the presiding Anima, as maintained by Stahl? Can it, however, be denied that this author is guilty of palpable inconsistency, when, after illustrating by nonentities and idle speculations the doctrines in question, he admits,

nevertheless, that increased or diminished impulse of blood is really an important principle of pathology, "though it has been carried too far." Is this critic prepared to carry this admission to its necessary consequences and corollary? Will he attach a similar value and importance, to all the supposed analogies, to the χωλη, the pituita, &c. &c.? It is evident, in fact, either that there is a total absence of analogy in the cases which have been introduced, or that the admission must comprehend these instances likewise, and that they, too, must be admitted as "of much "importance." Can any peculiar satisfaction be drawn from praise which is associated with censure so injudicious, and so contradictory? Or, can that be considered "a general principle of permanent "interest," which, in another place, is classed with the chimæras and reveries which have died away in their shadowy and unsubstantial nature? If systems have faded, which were founded on such unreal bases, must this equally perish, though built on a most important principle? This commentary is, then, either made up of vague and unmeaning asssertion, or the Elements of Pathology cannot, as the same writer elsewhere declares, "secure to Dr. Parry a place "amongst the medical authors whose works are to "be read for ages." As a subsequent opportunity will occur for exhibiting more in detail the real doctrines of Dr. Parry, in relation to these and other points, it can only be remarked, in this place, that, like some others, the present annotator declines shewing us in what particular instances the system

has been carried too far, and in which of the diseases enumerated, this common concurrent circumstance of increased quantity or velocity of blood, is incompatible, or has been found not to exist.

It has, on the other hand, been objected to Dr. Parry, that, instead of a Universal System, he has omitted the consideration of many parts of the animal economy, which are equally, with the Sanguiferous System, involved in the processes of disease; and that he has not included the enumeration of many diseases, of undeniable and paramount importance. How little this objection is generally applicable to the doctrines under examination, requires not to be pointed out to those who are acquainted with the Author's candid and comprehensive apology for the palpable imperfections of his System. How far it can be specifically applied to the large class of nervous diseases, and their connections, will hereafter be the subject of more particular inquiry. It may, however, safely be asserted, that no one was more sensible of his own deficiencies and omissions than this Author, although he has also, upon various occasions, alluded to important additions, which, in the prosecution of his plans, would have been made to his published works. If, however, certain Critics had taken the trouble to understand his actual doctrines, and if, instead of charging upon him what he has not done, they had, with accuracy, examined the nature and tendency of the opinions of which an exposition has been given, the cause of truth and justice would have been more essentially served, and

the public instruction would have been no less advantageously promoted.

That the Reviewers and Dr. Parry are often at issue on particular points of practice and pathology, will not be disputed. They are so amongst themselves; and it is not deemed of importance to reconcile all the discrepancies, which, from the nature of the case, must probably continue to exist. It may be left to time and experience precisely to determine the merits of the several parties. It is, however, a matter of obvious and palpable remark, that many of the supposed differences which serve as the basis of reproof or correction, are no less founded on haste and misconception, than is the general objection against an imaginary "Universal Cause." Is it true, for instance, that Dr. Parry's view of Epilepsy "does not take into account, nor attempt to explain, "those cases that arise sympathetically from denti- "tion, worms, or other irritations in the primæ viæ, or "from diseases of the spinal chord."* "Epilepsy," says Dr. Parry, "is often one modification of that "increased determination to the head which attends "dentition;" and again, "though it usually depends "upon excessive impetus of blood," he is "very far "from asserting that this, or convulsion, always arises "from such increased impulse of blood in the vessels "of the brain."† Has the Critic, from whom these observations proceed, so slight an acquaintance with the subject, as really not to be aware that the Author

* Ed. Med. and Sur. Jour. XV.

† Elem. pp. 249, 247, 302, 255.

places among its causes "heat, highly stimulating "ingesta, full meals, the internal use of some form "of alcohol?"* Has he never read, that "the effects "of different gradations of irritation or impulse on "medullary substance in living animals, whether of "brain, spinal marrow, or nerves, produce either "pain, convulsion, or the abridgement of the capacity "of sensation and voluntary motion;"† and, that "exostoses and other errors of structure, give rise to "this and other analogous complaints."‡ With what justice, or with what propriety has he then indulged in these crude and irrelevant remarks? The same question may, indeed, be asked in a variety of similar instances. In regard to Mania, which is asserted not to arise invariably from momentum, is not the sweeping objection founded on a view equally imperfect, if it be meant, that no other source of irritation is admitted, and that the abdominal Viscera are excluded by Dr. Parry, as causes by possibility implicated, in one part of the series of affection. It will, indeed, be the business of criticism to instruct us much more in detail, and with a much greater accuracy, as to the nature of its favoured "sympathy," and the "unknown" process of derangement of the sensorial function, before it can be considered competent to the decision of these questions, or be permitted, by unmeaning and inapplicable declamation, to set aside the specific relation which is attempted in the experiments and investigations of Dr. Parry. But this latter Author himself declares

* Elem. pp. 261, 276. † P. 291. ‡ Pp. 252—3.

of Mania, that "many other causes, different from "sanguineous impulse, and mere impetus of blood, "produce a similar effect."*

Is it true, again, that, according to another Critic, he has "barely glanced at the subject of Hæmorrhage "from the Uterus and Lungs?"† Many things are certainly omitted which, in the subsequent volumes, would have deserved and occupied a more ample space; but as these diseases have not altogether escaped the Author's attention in his present works, it is probable, that they have, by him, been sufficiently noticed for his immediate purpose. We have, indeed, as has before been remarked, still to learn, with regard to many cases in which a difference of opinion may exist, whether a greater reliance is to be placed on the knowledge of Dr. Parry, or on that of his commentators. His experience, for example, was decidedly against the opinion, that Dropsy is primarily induced by a diminished power of absorption, as distinguished from the manifest effects which may be ascribed to the circulating system. "Though such "a cause is theoretically possible, his observation had "not furnished him with a single fact in direct proof "of its agency."‡ It is, indeed, somewhat bold to place in opposition to this experience, the theory that "after protracted inflammatory complaints, or "other debilitating causes, a deficiency of absorption "actually does occur, partly from torpor of the whole "lymphatic system, and partly from the impaired

* Elem. pp. 275—7.

† Lond. Med. Rep. vol. v. p. 115. ‡ P. 123.

"tonicity or contractility of the cellular texture "throughout the body, by which, when not impaired, "the circulation in the vasa minima is so greatly pro- "moted, to say nothing of the yielding state of the "capillaries themselves under such circumstances."* That a debility of every part of the system may be the result of continued inflammatory or other morbid conditions, cannot well be denied. But are the cases correspondent? Is the supposed disease parallel to that in which many pounds of Serum are effused into the abdomen in the short space of twenty-four to thirty-six hours? It remains, at least, for the Reviewer to shew that the diminished absorption is a primary cause of the symptoms; and that, if no previous inflammatory affection had existed, similar effects would also have arisen from an original and positive, as they may, in the present case, from a secondary and relative, diminution of power. But Dr. Parry admits the possibility of this theoretical case, and we must depend upon the experience and observation of others for an accurate distinction between the specific effects dependent upon the admitted previous states, and the general consequence resulting from a positively diminished capacity in the absorbing vessels to take up the effused fluid, whether this be of a natural, or of an unhealthy quality.

That some of the objections which have been advanced against Dr. Parry, have their origin in the causes, which during many years delayed the public avowal of his opinions, might easily be made manifest.

* Ed. Med. and S. Journal, vol. xv. p. 93.

His particular doctrines, at a period when they were most opposed to general and individual feeling and habit, were widely dispersed in correspondence, and by the communication of his written cases. It has already been observed, that many of his sentiments were thus adopted, and even published by Authors who had no claim to originality of conjecture, or to priority of observation. It is very far from the Editor's wish to assert the same with regard to Dr. Blackall on the subject of Dropsy. Nothing can, however, be more easy than to prove from written evidence, and the personal testimony of a very large number of medical practitioners still existing, that the inflammatory nature and connection of this disease were taught by Dr. Parry, and adopted as the foundation of his practice, twenty years before the publication of Dr. Blackall's interesting work.*

The doctrine of the general salutary tendency of disease has no where been claimed by our Author as original or novel, though, in some individual and unexpected varieties, the particular application of this principle may have been so asserted. The universality of the fact was no more maintained by him than its exclusive discovery. Neither, under such a modified adoption, is it with him, as certain Critics suppose, a necessary conclusion, that fatal disease does not exist as a tendency of our animal constitution; though, in many instances, what are called diseases, are, in fact, processes of a salutary kind.†

* See various cases published on this subject in the Collections.

† Elem. pp. 103, 111.

If inflammation, hæmorrhage, dropsy, convulsion, within certain limits, beneficially relieve the constitution from a previous morbid affection ; they are, under other circumstances, equally the causes of morbid states, and of death itself. It certainly does not follow, that because all diseases are not salutary, some morbid conditions may not be so, nor that where nature had a general protecting influence in view, she may not sometimes fail in consequence of particular opposing arrangements. That all diseases do not prolong life, needed not have been told us.* No doubt can, indeed, be entertained that the final cause of a great majority of the diseases incident to humanity is to be sought for in the direct appointment of Providence. It might be safe to proceed a step farther, and to admit, that all diseases, even those which are fatal, are in fact so ordered, and "like other physical evils, are "intended to fulfil certain inscrutable purposes, both "moral and natural ; of the latter, perhaps, the only "obvious one being the removal from this stage of "existence of the successive generations of mankind." All this, it may be repeated, was superfluous as directed against the Author of the "Elements," since the Critic himself states it to be a fundamental position of Dr. Parry, not that all, but that "most "diseases" tend to restore and prolong life. He does not, however, seem justified, even in the ample scope which he has permitted himself in this statement. Dr. Parry enumerates "Hæmorrhages from various "parts, Cutaneous Diseases, Nervous Affections,

* Ed. Med. and Sur. Jour. l. c.

"Inflammations, Gout, Dropsy, and other increased "Secretions and Excretions, as among the movements "constituting what is called disease, but which are, in "reality, intended for the relief of the overburdened "and oppressed constitution; of which movements "the result may, however, be salutary or otherwise "according to certain specified conditions."* How accurately has this Author himself answered the objections, and how pointedly has he denied the salutary nature of many morbid processes, though, according to his belief, "many of these movements"† have a tendency to restore and prolong life. Without deviating into a consideration of moral final causes, will the Reviewer take the pains to contradict the specific cases, and demonstrate, that, in these, at least, Dr. Parry's doctrine is founded in ignorance, and supported by error. This Author neither denies the existence of physical evil, nor asserts that "most" diseases have a salutary tendency. If the seeds of its own waste and consumption are planted in the animal body, so are, also, for its own limited preservation, the principles of repair and renovation. The question under consideration relates to the physical final cause of certain morbid movements. Are dropsy and effusion a relief to inflamed and over-distended vessels? Does hæmorrhage relieve congestion? Is convulsion a process tending to restore an equilibrium which has been destroyed, especially in the functions of the circulating system?

* Elements, p 363—5. † P. 371.

To some minor points, which have above been noticed, the Editor does not, at present, think it necessary to direct his attention. They may be well left to the scrutiny of time. To subsequent experience and advancing knowledge, it may also be left to estimate rightly the doctrines of Messrs. Gall and Spurzheim, which, with no unqualified praise, Dr. Parry admitted, not as the basis, but as the occasional support of some of his conclusions. Some other subjects connected with these enquiries, the Editor has thought it necessary to examine in a much more precise and detailed manner. Under the heads, Irritability, Inflammation, Determination of Blood, and Nervous Diseases, will probably be found some remarks and illustrations which have reference to points, imperfectly on that account, noticed in this place. Where so many eminent and gifted men are concerned, on subjects in which the meanest capacity may discover a flaw, and in which the highest endowments may fail to establish the truth, far is it from the Editor's disposition to find fault, and to correct in haste, or in vindictiveness. He will be excused for an an attempt, in connection with the re-publication of Dr. Parry's former works, and the publication of much new matter, to rescue this Author from objections which appear to have been advanced in haste, in ignorance, or in error. He is himself open to similar charges, and will endeavour, on such future occasions as may offer themselves, to meet those which are fairly urged, without ill-humour, and with an openness to conviction. If, in the execution

of a work of this magnitude, he must inevitably fall much beneath the public expectation ; he is still more remote from the accomplishment of his own wishes, and below his own ideas of the importance of the task, which has, unhappily, devolved upon his sole charge and responsibility.

It remains to give some account of the materials which are in the Editor's possession, to state some of the difficulties which have attended their examination, and to submit the ground upon which a selection for publication has been made. That these Collections are very large, will not be doubted, when it is mentioned, that from the commencement of the Author's studies, through his long career of practice, to the period of his remediless attack, every case or observation of interest was immediately recorded. The loss of no opportunity was allowed, even under circumstances, in which an excuse for the omission would generally have been sought, and admitted. Notwithstanding a very extensive practice, which for many years occupied twelve or fourteen hours every day ; notwithstanding the constant variety of cases which daily occupied his attention, and induced a continued mental anxiety, he constantly persevered in the practice of noting down, on foot, in his carriage, and even in his bed. His daily lists are accompanied by detached remarks, or connected cases, and his chests are filled with collections of a miscellaneous kind, chiefly in an unconnected form.

A principal difficulty attending this mass of papers has been occasioned by the distant periods at which

the various observations were made. Many of them are as early as the year 1776, and it will be hereafter shewn, in reference to certain proposed experiments connected with the Nervous System, that even those earliest manuscripts are not devoid of interest. In consequence of the remote period to which they extend, a doubt has arisen as to the value which should be attached to these earlier records, as part of a series, which, under a continued and improving experience, might, even in the Author's own view, have been less fit for public inspection.

Without further reference to this difficulty, which has required a minute and tedious examination of the numerous documents, it may be briefly stated, that the selection of the present papers has generally been made on one or other of the grounds shortly to be mentioned. It is, indeed, fearlessly acknowledged, that every fragment has been considered of more or less value, which has proceeded from an experience so extensive, watched over by a judgment so powerful and so correct, and which was so carefully assisted by the habit of immediately noting the peculiarities and accordances of the numerous cases which were daily presented to the Author's observation. Where every thing is original, and derived from such a source, there cannot, it is imagined, be a question as to the importance of the matter, whether it now appear, for the first time, as a novelty, or confirm only the prior experience and remarks of other practitioners.

It is, however, also readily acknowledged, that, wherever dates could be obtained, one ground for

selection has been the support of that claim to priority of observation, which has so often been lost by the delays connected with Dr. Parry's professional habits. Such an opportunity has not been neglected, even though the subject has no longer the recommendation of novelty. However really unimportant in itself, an attention to this point has appeared in some measure due to the merits and to the reputation of the Author.

First, then, a selection has been made with reference to the period and priority of the subjects, whenever such a knowledge could be obtained; and it may, for once, be remarked, with regard to the present and subsequent divisions, that, though the general reasons for publication are specified in this place, the matter is not distributed according to this formal arrangement, but occurs, in a dispersed state, under the several heads to which it immediately belongs.

Secondly, a selection has been made of those facts and observations which confirm, or place in a new or stronger light, information which has already been made public. Such repetition seems particularly required in the case of an Author, who has been frequently, and in a very important degree, misunderstood, and misrepresented. We thus enable him to defend himself with his own weapons, to put on his whole armour of protection, and to appear in his own strength and shape, against the array of his opposers.

Thirdly, it has been the Editor's wish, to preserve all such facts and observations, as do not occur in any

of his printed works. Such an addition will constitute matter of much interest, as proceeding from so judicious, and so diligent an observer.

Fourthly, he has been particularly anxious to record and communicate every thing, which related to Dr. Parry's Treatment of Diseases, respecting which so little has been published, and in regard to which so many errors have been adopted. This part of his task has, in many respects, been considered the most important, and the Editor has, without fear, presented to the public a collection of facts, which, under other circumstances, might be considered too trivial, or too voluminous. Of several hundred cases, he has thought it adviseable to publish as many as were correctly written, in relation to diseases of interest and importance, and especially to retain all such as were accompanied by dissections. He offers no excuse for the extent of this principal part of the present volumes. His own feelings lead him to believe, that, under the circumstances, such a record of Dr. Parry's industry will, at least, not be injurious to his reputation ; and that, while it corrects the prejudices or misapprehension of many readers, and illustrates many principles cursorily noticed in his former works, it will direct to beneficial results those, who, with discrimination, follow the practice in these instances recommended.

Fifthly, there are many detached fragments which could not easily be classed under separate heads, which the Editor has, nevertheless, thought it right to preserve, as a collection of Miscellaneous Papers.

Of these, the principal is a Preface, which was intended for the larger work, and which was written in 1811. It includes a general inquiry into the objects of human knowledge, and the means of attaining truth, with particular reference to the Science of Medicine, and to the Author's own opportunities of experience, and improvement, during a long career of professional occupation.

Of the difficulties, which the Author felt in arranging the plan of his proposed work, he has, elsewhere, spoken in the following manner:

"In a work, the object of which is to investigate the "circumstances which produce pain, shorten life, and "impede the free exertion of the mental and corporeal "faculties of man, it is obvious that our first inquiry "should be into the nature of those faculties "themselves.

"Here, in the very outset, on which side soever we "turn, we are surrounded with difficulties. With "what part of man shall this important inquiry com- "mence? Shall we begin with his corporeal frame, "describe its mechanism, and ascertain its laws? But "these laws themselves are not only connected with "mind, but are evidently in a great degree dependent "on its influence. Should we, nevertheless, determine "on a previous investigation of the body, what is there "which shall direct the order of our choice to the "Nervous or the Sanguiferous System? If, on the "other hand, we are disposed first to examine the "properties of mind, we should consider that this work "is not intended as an express disquisition on ethics

"or metaphysics, but that it professes to touch on "both sciences merely so far as a knowledge of them "may be subservient to a healthy administration of "the bodily and mental functions.

"In reality, the whole animal system is a sort of "chain, in which the several links are so disposed as "that each, in its turn, acts as a cause upon every "other link, and as an effect, is acted on by it; and "in which no beginning, no origin can be traced, "except to the all-present and omnipotent mind "of Deity."

Sensible of the uncertainty, under which the Author still laboured, with regard to the most perfect distribution of his materials, and perceiving the insufficiency, for a more extended work, of the scheme already adopted in the "Elements," and even in that more detailed catalogue of diseases, which were classed under the head of increased determinations,* the Editor has experienced considerable doubt and difficulty. He has finally chosen that arrangement, which seemed to him the least liable to objection, and which, while it avoids all compromise of the Author's principles and views, can occasion no cavil as to the accuracy of any hypothesis, or the truth of any system, and is, at least, as comprehensive, as either of the schemes already noticed. He has, for these reasons, preferred a classification, which distributes the various diseases as nearly as possible under their respective seats of affection, however different may be the nature of such affections them-

* P. 320.

selves. Thus, under the head Fever and Inflammatory Affections, Affections of the Skin and Integuments, Affections of the Head and Nervous System, Thoracic and Cervical Affections, Abdominal and Pelvic Affections, &c. will be included all the varieties, whether the simple inflammatory conditions themselves, their consequences in effusion, hæmorrhage, deposition, or the related forms of disease, which, without too minute a subdivision, could scarcely be included under a more precise and distinct arrangement. In this view, Gout and Rheumatism, which may be said to belong equally to the muscular and to all the textures implicated in the functions of the joints, and to have a general as well as a local relation, are described under the head first mentioned; and Dropsy, every where finds its place, according to the different form which it assumes, and the different seats which it occupies. As, again, under Cerebral Affections, are included all Affections of the Nervous System, which is so nearly connected with the Brain; so, also, under Thoracic Affections, is admitted all that relates to the Sanguiferous System, which has so near an alliance with that important viscus, the Heart. This arrangement is very incomplete, and is, as before remarked, adopted rather as a matter of convenience, than as claiming pretensions to a philosophical or systematic accuracy.

The Pathological and Therapeutical Collections are preceded by some observations, on the elementary subjects of Anatomy and Physiology, which though few and imperfect, it was not deemed adviseable

altogether to suppress. In relation to the former branch of science, much, indeed, has been withheld, which, though intended for publication, and derived from the Author's immediate observation, was not considered of sufficient interest, whether from peculiarity, or from novelty, to warrant, by its introduction, a considerable extension of the present work.

In conclusion, it may be honestly declared, that the Author's friends are not very anxious as to the merits of priority of discovery. They would contend for these, the last of all their points of defence. They are anxious to establish distinctions which have been confounded, to remove objections which are frivolous or inapplicable, to explain opinions and doctrines which have been mistaken, to expose declamation which has been sometimes substituted for argument, and to weaken an opposition which has arisen from an attachment to previous conceptions, and been fostered and sustained by habits of indolence or of error.

They are not anxious for an immortality,* to which, consistently with its native infirmities, humanity cannot aspire. They are prepared to be censured, perhaps injudiciously praised, and to be partially forgotten. But, they are willing to leave to time, and the clearing away of the mists which naturally gather round contemporary systems, to embody or to disperse those forms, to which they have been accustomed to attach their faith and their admiration. To this ordeal, they willingly consign the correction

* Edin. Med. and Sur. Journ. l. c.

or the confirmation of opinions, which, to them, have appeared consistent with the real order and quality of phenomena, and superior, in their practical value, to the doctrines of an inscrutable agency, and of certain unseen and incalculable effects. That this system is very far from perfection, is a trivial and irrelevant objection. A perfect sytem was never contemplated, and a more complete arrangement was prevented by the decree, which arrested its Author in his mid career of industry and observation. The Architect is no more! A few years would have consolidated into a more noble structure the fragments which are impressed with his feelings, and with his energy. These scattered masses are collected together, and if they do not form a monument entirely worthy of his name, they will still, it is hoped, constitute a heaped memorial of some beauty, and of so much durability, as may preserve even his human existence from a merciless and enduring oblivion.

CHAPTER I.

IRRITABILITY.

ALTHOUGH the question of the irritable nature of the vascular system, and its function of assisting in the circulation, was ably discussed by Dr. Parry, both in his "Elements of Pathology," and in his "Experimental Enquiry," and has also been briefly considered in the "Additional Experiments," the stress which, since the period of those publications, has been laid on certain arguments, and experiments, seemingly favourable to the opposite doctrines, demands, at the present time, a more detailed examination of both these grounds of dissent. The great speculative interest of this subject of physiological enquiry will, of itself, justify an extended attempt to disentangle it from the errors which seem ascribable to irregular deduction, or to inaccurate observation; and its intimate connection with the most available points of Pathology not only excuses, but solicits our attention, in investigating a practical

and highly important truth. If we must still confess our inability to penetrate the hidden things of nature, and be, in our turn, convicted of the weakness with which we may seem to charge our opponents, this renewed investigation will, nevertheless, have its use, and we shall contemplate with pleasure any result, whether favourable or adverse to our own opinions, which may place the connected evidence on a basis secure, and accessible to the advances of future enquirers.

It may be well to premise in this place, that the subjects of the real Power of the Heart, and of the Causes of the Circulation of the Blood, are not considered essential in the investigation of the immediate question, however they may have been generally associated with it, as matters of physiological interest, or of speculative importance.

The Irritability of the Arteries of warm-blooded animals has lately been maintained on three distinct grounds. First, their contraction under exposure, and the irritation arising from the circumstances of the experiment: Secondly, an increased pulsation, or contraction and dilatation, which appears on the application of chemical and mechanical stimuli: Thirdly, the retrograde motion, which is manifest when the circulation is interrupted by a ligature, or by other causes of obstruction. These facts are supposed to prove in the arteries a property identical with that, which, in other parts, is called Irritability, and which is defined a contraction on the application of a stimulus. It was the opinion of Dr. Parry,

that such effect was not made manifest by such means, and that there was some difference, therefore, in this respect, between the power of arteries, and that which is peculiar to parts confessedly irritable. It is true, that, in some instances, he notices a contraction on the exposure of arteries. In others, such occurrence did not take place, even under the influence of friction, and notwithstanding various causes of excitement. In these cases, the vessel remained, under all circumstances, "equally full and extended," and without indicating any change of diameter. Of this occasional appearance of contraction so many causes might be admitted as alternatives, that it appeared unsafe to draw, from such partial effects, any conclusion as to the general irritability of the vessels, the existence of which was, at the same time, disproved, by phenomena bearing no analogy to those of parts endowed with this property. The alternatives which might exclude any direct vital agency of the vessels in the production of this appearance, are, among others, the usual and corresponding effects of cold upon parts confessedly irritable; the effects of cold, also, on fluids, and, possibly therefore, on the contents of the vessel so seemingly affected, together with the acknowledged tonic contraction of the vessel, under such a state of the contained blood; a modification of the tonic contractility of the vessels, as in muscular parts, apparently endowed with this faculty; increased diameter of the vessel in a preceding seat from the distending influence of the blood, and the contraction which follows in a subsequent

seat from the changed momentum, whether from altered velocity or quantity; the particular effects of position, and of altered gravitation, producing, also, perhaps, in some preceding part of the vessel a dilatation, followed, as before, by a tonic contraction on the reduced local amount of fluid in the preceding portion, &c. &c.

In examining this question of the irritable contractile power of the arteries in warm-blooded animals, we can scarcely, indeed, avoid feeling some degree of bias towards the opinions, and experience, of the great discoverer of Irritability, and of many other eminent men. The Reponse à la Critique de M. Whytt,* by the first mentioned author, will satisfy those who are desirous of pursuing the subject, that the best supports of the adverse doctrine rest on a weak basis; and that theory, which, notwithstanding the multiplication of experiments, is still its fundamental support, will not avail in deciding this difficult point of physiological enquiry. "If," as Haller says, "in regard to Irritability, contraction "be the action of vessels, it should empty, and not "fill them, as it is supposed to do in the hypothetical "cases of Whytt;"† and, if cold will contract the skin and tunica dartos, the instances do not apply to the doctrines of Haller, who, from experiment, has excluded them from the parts called irritable, and which contract on the application of a stimulus.

* Mem. sur les Parties Sensibles et Irritables, par Haller, vol. iv. p. 99.

† Haller Op. Minora, vol. i. p. 189.

The varieties which occur on the exposure of arteries to the external cold, without the application of any direct stimuli, or when, according to the analogy of irritable parts, the effect of the stimulus of the knife may be supposed to have ceased, are too well known to require repetition. There is scarcely any diversity of state which does not occasionally, sooner or later, and in a greater or less degree, occur. The usual effect in the part so exposed is, at first, a dilatation, and, subsequently, a contraction, even to an almost impervious condition of the tube.

In the Additional Experiments on the Arteries,* the solution of muriate of soda appeared, in a single instance, to dilate the Carotid of a Sheep. This dilatation seemed afterwards to be removed by the application of Liquor Ammoniæ, and a contraction to be induced to the extent of $\frac{50}{400}$ below the first state, which, however, was again restored by the use of the Salt. To shew how equivocal as to the irritability of the vessel, or its contraction on a stimulus, such an experiment must be considered, the following repetitions of it are subjoined, which also exhibit the simple effects of exposure on the same vessel.

I. The pulse of a Wether Lamb being 120, and the respiration 66, the right carotid was exposed. In consequence of the division of a small arterial branch, much hæmorrhage occurred, and considerable irritation took place, before, from its immediate

* Exp. viii. p. 13.

proximity to the trunk, an effectual ligature could be made.

1. The vessel then measured ..... $\frac{170}{400}$.
2. After exposure for several minutes, ... $\frac{210}{400}$.
3. In two minutes, Spirit of Wine was applied, when the measurement was $\frac{200}{400}$.
4. In five minutes, on the application of Liq. Ammoniæ, the vessel measured $\frac{181}{400}$.

The animal was becoming faint; p. 144, resp. 100.

5. Sat. Sol. of salt being applied after an interval of four minutes, the measurement was ... . $\frac{170}{400}$.

The respiration was very irregular and intermitting, sometimes with a long interval.

6. In five minutes the Liq. Amm. was applied, when the vessel measured $\frac{160}{400}$.
7. On the application of Sol. of Salt, in ten minutes after the last trial, it measured $\frac{160}{400}$.

In this instance, the decided effect of exposure, and irritation, seemed to be a dilatation of the vessel through the whole length exposed, which continued, after the Spirit of Wine had been applied, for two minutes. The Liq. Ammoniæ and Salt gave considerable pain, and this, together with long continued irritation, produced much exhaustion, and ultimately faintness, which might have proceeded to the death of the animal, at all times little able to bear the unnatural posture required in these experiments. During this whole process, not the slightest movement of contraction and dilatation was perceptible from exposure, the application of the ligature, or

that of the various stimuli. Shall we conclude from this experiment, that dilatation is the usual consequence of exposure, or that the Liquor Ammoniæ produces all the effect of contraction, while exhaustion and other causes have no co-operating influence in producing these perceptible states? The salt, in this instance, did not dilate the artery, and, like that of the Liquor Ammoniæ, its application was only coincident with an effect, by which the artery was restored to its original caliber.

II. The right carotid of a Six-toothed Merino Ram was exposed. P. 120, R. 42. Throughout this experiment two measurements were constantly taken, *a* being in a spot nearest the heart, *b* in a situation nearest the head.

			INCH.
1. Immediately on exposure,	*a*	measured	$\frac{270}{400}$.
	b	$\frac{240}{400}$.	
2. In fourteen minutes, ...	*a*		$\frac{290}{400}$.
— two more, ...	*b*	$\frac{281}{400}$.	
3. In ten minutes, ...	*a*		$\frac{330}{400}$.
— four more, ...	*b*	$\frac{310}{400}$.	
4. In ten minutes, ...	*a*		$\frac{310}{400}$.
— three more, ...	*b*	$\frac{280}{400}$.	
5. Liquor Ammoniæ was applied:			
In five minutes, ...	*a*		$\frac{270}{400}$.
— two more, ...	*b*	$\frac{240}{400}$.	
6. Sat. Sol. of Salt was applied:			
In four minutes, ...	*a*		$\frac{220}{400}$.
— three more, ...	*b*	$\frac{192}{400}$.	
7. Liq. Amm. applied:			
In four minutes, ...	*a*		$\frac{280}{400}$.
— two more, ...	*b*	$\frac{200}{400}$.	

8. Salt was applied :

In two minutes, ... *a* $\frac{210}{400}$.

— two more, ... *b* $\frac{180}{400}$.

From the period of No. 3, the animal had shewn evident marks of exhaustion.

That, in this instance, a marked difference existed between the first and the last states of the vessel, is very evident; but it was an effect not in accordance with those usually observed under the influence of apparently similar causes. Under exposure, the artery either continued in its original state, or became dilated, while the contraction advanced equally under the use of Salt, or of Liquor Ammoniæ, and the greatest degree of contraction existed under the influence of the former stimulus. Were the effects so accurately and distinctly connected with the application of the Salt and Ammonia, as to exclude the influence of a very cold air on the tunics, or on the blood, or the possible diminution of momentum under a state of exhaustion? If not, what can we learn definitively as to the specific Irritability of vessels, from experiments, in which the results are necessarily so unequal and various.

The Experiments published on this subject by Dr. Hastings,* shew a still greater diversity of effect, such a diversity, indeed, as entirely to vitiate any conclusion as to the real action of the vessels. In determining this natural action, can it be considered an immaterial point, whether a contraction be pro-

* Introduction to a Treatise on Inflammation of the Mucous Membrane of the Lungs. 1820.

duced, or a dilatation, or a contraction and dilatation, or a tortuous and vermicular motion? Yet such are the varieties which, with all their alternatives, are classed under one head, and as occurring in these experiments, under nearly similar circumstances, are considered the effects and proofs of the same power, Irritability? Besides a fair and legitimate doubt, as to the real power of the vessels, which must arise from such discrepancies, we may also safely enquire, in passing, whether the fact, that one or other of these occurs occasionally, and none uniformly, that the very opposite states make a part of the series of phenomena produced by the same apparent causes, be not an evidence that no one of them is a necessary property of the vessels for any uniformly proceeding function? Nothing can be more variable as to effects, more unsatisfactory as to proof, or less conclusive as to the powers of the vessels, or their real influence in the circulation, than these experiments. If, indeed, any local change which may occur in vessels from the application of a stimulus, warrant the adoption of this peculiar power, Irritability must be otherwise defined than a contraction on stimulus. If the same stimulus produce at one time contraction, at another dilatation, as happens in these experiments; if, "in "one instance, the contraction occasioned by the "Liquor Ammoniæ was $\frac{1}{8}$, and in other examples it "produced an opposite effect," it is not to be expected that we should concede the point at issue, and agree that the latter effect is the consequence of Irritability suddenly destroyed. Under nearly

the same circumstances, the effects should not be the reverse, if the relation of cause and effect were that through which the changes are effected. And how does this diversity of effect afford any analogical argument in favour of an identical power? There is a peculiar action in the heart, in the intestines, bladder, muscles. Does the same stimulus in these produce such opposite effects? Is there any doubt as to their appropriate action when stimuli are applied? or would it not be just as fair to attribute any varieties in their action to another faculty than that of Irritability, as to ascribe this property to vessels which exhibit such irregular and anomalous motions? May not the tortuous and vermicular movements of the intestines, for instance, be ranged, with as much probability, under a class distinct from their power of contraction on a stimulus? or, may not their structure, essentially differing from that of the arteries, for the purposes of a peculiar function, admit a complex effect of Irritability, without sanctioning any analogy from mere external similitude between their action and that supposed to belong to the arteries? But even this similitude fails altogether. What nice observer could commit himself by a comparison of the vermicular arterial movements with those of the intestines? The diversity of their conditions is as great as that of the relations and objects of the parts in question; and perhaps, in the whole enquiry, no instance can be supplied as clearly distinctive of the nature and functions of parts, as the difference in the movements and purposes of the arterial and intestinal tubes.

It will not, surely, be argued, in a case, where, the very existence of the faculty of irritability is the matter in dispute, that it is of so refined and exquisite a kind, as to combine, in the vessels, every specific irregularity and change, which may happen in parts decidedly irritable.

The present occasion will not admit a complete analysis of the experiments and arguments, on which a reliance is placed by the authors who maintain the opposite doctrine. A few observations drawn from their general results, and from a few detached experiments, must suffice in this place to illustrate and confirm the preceding remarks. The following result of a Table,* supplied by Dr. Hastings, will shew that they have not been hastily or incautiously made. By the application of stimuli, contraction is said to have been produced, sooner or later, greater or less, in seven arteries and eleven veins. In eleven instances, no contraction or other effect was perceptible. In ten instances, the vessels were much dilated, and in three the circulation was much quickened. So that, admitting the very disputable point, the evidence of contraction from increased velocity, the proof from such examples is only equally balanced on both sides.

It might have been considered sufficient, as in the Additional Experiments, to leave to Haller himself a reply to the arguments drawn from the experiments of Verschuir, which are supposed to have been the most accurate that have been recorded, and which,

* Tab ii. p. 57.

in the time of the first mentioned Author, were particularly opposed to his views of vascular irritability. It is very evident that they produced no conviction in his mind, with whatever liberality he may have treated their author. It may, however, not be irrelevant to notice, that giving the greatest latitude of admission to the recorded results of these trials, they cannot be considered at all decisive on the subject. Of twenty-one experiments, ten are not in favour of the supposed fact, and the preponderance of a single instance in a case where an important truth was under the examination of a philosopher of zeal and ability, will not carry all the weight which was wished, or which was intended. It is, indeed, said, that, in a single instance only, this effect was so manifest, as to convince the most sceptical "*manifestissime semel.*" But is it quite clear to those who have so often repeated these experiments, that such contraction, once only so decidedly manifest, might not have occurred, in an exposed artery, without any irritation? Many such palpable examples must be adduced, before the experiments of Verschuir can be considered decisive of the existence of vascular Irritability.

But taking the separate proofs of a vital irritability or power of contraction in vessels, what clear inference can be drawn from the following experiment of Dr. Hastings? After scraping the femoral artery of a cat for twenty minutes, contraction and dilatation of the vessel appeared, and in half an hour's time "a slight *permanent* contraction" was

induced. What analogical instance can be produced in proof that these effects were those of irritability? Scrape the heart, œsophagus, &c. and see if there be any similitude of effect. To say nothing of the contraction and dilatation, of which there will be occasion to speak hereafter, in what irritable parts is the analogy to be found for a permanent vital contraction corresponding with the effect here described? Does not diminished temperature alone produce such a change in some instances? Is not the permanency of the effect more like that attributed to Tonicity, than to any known effect of irritability? tonicity, a power which can permanently close and obliterate the tube which has been the subject of its extraordinary contractile influence? Again, if, as is afterwards assumed, the contraction and dilatation be the evidence of excited or increased power, the contraction peculiar to irritability cannot likewise be such a condition. Both cannot be adduced as proof of the existence of this faculty. In this instance, the contraction seems to have been the consequence of exhaustion from over-action, which probably the advocates for dilatation, as an indication of debility, will not admit. These authors are, however, sometimes inconsistent with themselves. With them, even "in "vessels much dilated," a "slow oscillatory move-"ment,"* itself the proof of power, sometimes appears.

These and similar remarks may be applied to various other experiments which have been recorded. A quarter of an hour after contraction

* Page 85.

and dilatation had been produced, in another instance, the vessel appeared contracted $\frac{1}{10}$ of an inch.* Contraction, the natural effect of irritability, is here again the sequel to another anamolous display of phenomena, which, as a local effect, intervenes between the cause, and the specific consequence, the increase of natural action. The only unequivocal, and it may perhaps be said immediate, instances of the effect of contraction from stimuli, are experiments† No. 1, in which it occurs after three minutes irritation: No. 5, in which permanent contraction is induced after ten minutes: No. 7, in which irregularity of surface from contiguous contractions occurs after fifteen minutes: Nos. 24 and 25, in which immediate contraction followed the application of nitric acid: so that in these instances the nitric acid alone produced this effect, which it will also excite chemically, in other parts not endowed with irritability.

With regard to the period of time, which elapses between the application of the stimulus, in some of these experiments amounting to one or two hours before the supposed influence is felt, will it be said that, in these cases, no doubt can be entertained as to the cause of the production of the effect? Will it be pretended, that the usual permanent state of the contraction implies no difference between this condition, and the wholly disimilar effect visible in irritable parts? It may be urged, as an apology for the absence of any perceptible effect, or the want of a uniform result, that different stimuli produce

* Page 25. † Table i. p. 27.

different effects on different parts, and act differently at different times. This may be true; and yet that this singular coincidence and uniformity of dissimilarity should have place only in the vascular tunics, should somewhat qualify the conclusions we might be disposed to draw, as to the identity of this property with that common to irritable parts. Take out the heart, œsophagus, stomach, intestines, bladder, and a piece of the aorta. Irritate them separately with the point of the knife, with liquor ammoniæ, or any other stimulant, and then say where the similarity exists, or what analogy can be adduced in proof of an identical power. These viscera do not refuse to obey the stimuli, but contract immediately on their application. I know no more beautiful or more convincing proof of the very low degree, or entire absence, of irritable contractility in the arteries, than the difference which may be seen in the effects of touching with the point of a knife a piece of the œsophagus, and a corresponding portion of the aorta of a rabbit. The former is immediately put into motion, the latter remains perfectly quiescent. It happened accidentally to the present author thus to touch a piece of the œsophagus, which he believed to be a section of the aorta. The immediate and palpable effect astonished those who were witnesses of the experiment, and the doctrine of arterial irritability seemed to be completely established. Nothing could, however, be a more decided refutation of it, than the discovery of the truth, and the immediate comparative trial which was instituted with the

aorta itself. In all the experiments adduced, the alternative effect on the contained blood is entirely kept out of view, and the question, in itself and in its analogical relations most important, is omitted, whether, in the artery emptied of its blood, such effects or such contraction can be produced, as, under similar circumstances, are witnessed in other irritable parts? This would, in fact, be the true test of resemblance with a power peculiar to these parts of the living animal body.

It has already been observed, that, in these experiments, contraction is not the universal occurrence.* In some instances, after an increase of pulsations, the vessel dilated on the application of a stimulus. This mechanical or hydraulic effect is such as we expect, and is, probably, a very common one in health and disease, of causes giving rise to an increased impulse, or momentum, in one or more vessels, either from the heart, or from local changes in the vessels, without increased action of the heart. Here, again, we have an important alternative in the probable changes which occur in the blood itself under such circumstances, as we shall have occasion farther to notice, when considering the important subject of inflammation.

Deserting the specific effect of irritability, contraction on the application of a stimulus, authors have adopted increased pulsation itself as evidence of the irritable nature of arteries, or as indicative of the power inherent, and excited by a stimulus, in these

* Exp. 22, 23, Table i.

vessels. By pulsation is understood an increased contraction and dilatation of the vascular tunics. On a first view, we may be disposed to enquire, how, if increased contraction be a manifestation of power, increased contraction and dilatation, as a strictly local effect, and as unconnected with any assistance from the heart, can also be such a manifestation of power. With regard to the fact itself, it is scarcely necessary to remind the reader, that, in this sense, neither Dr. Parry, nor any of those who assisted in his experiments, ever discovered this pulsation, under circumstances of exposure, or under any influence of a direct stimulus applied to the vessels. His explanation of the appearances simulating such motions is familiar to all those who are conversant with the "Experimental Inquiry," and who have been accustomed to examine the arteries of living animals. It will, however, be admitted, that an effect, somewhat analogous to that described, might occur under peculiar circumstances of the vessels, and under an influence positively, or relatively, increased or altered, of the heart, or even of certain vessels, which, by increased impetus, forcibly distending the succeeding arterial tubes, might elicit a superior exertion of their admitted tonic or elastic powers, and tend to restore their mean and natural caliber. Neither will it be wholly denied, that, in seats contiguous to the heart, where perhaps some similarity of structure is continued, an effect somewhat analogous to the action of the heart may take place; though this is rendered improbable by the

circumstance, that the uniform motion of the blood does seem chiefly dependent on the heart, and not, as in the assumed case, on any alternating or compensating action between the heart and arteries. If you suspend the heart's action by ligature or otherwise, in far the greater number of cases, the motion of the blood is carried on by jets, and has no longer the uniformity which belongs to its natural state.* It has, elsewhere, been admitted, that, in the superabundance and providence of nature, a vicarious action, may, under certain circumstances, be assumed, and a substituted power be attached to the arterial system, which, on ordinary occasions, is the exclusive and peculiar property of the heart.

In considering the subject of increased momentum as possibly productive of local dilatation, it may not, on this occasion, be irrelevant to repeat the observation, that this is not necessarily connected with changes in the heart's action alone, but, that new sources or modifications of momentum may be derived from the admitted changes occurring in various seats of the vessels themselves, from causes external or internal, and that these effects themselves again become productive of varieties of motion. Neither must we forget, that the same cause of motion may become relatively excessive, under these and other varieties of condition. Under obstruction from pressure, or distention in series No. 3, the state of No. 2, will be materially changed by the common action of No. 1, which remains precisely at it was before. In certain

* Exp. Inq. and Addl. Exp.

conditions of the tonsil, the carotid artery, which has few anastomosing branches, or a number insufficient to permit a rapid escape of its blood, will suffer all the possible changes which can arise from distention, and the effort at recovery, under an influence from the heart, now become relatively excessive to its power of circulating the blood. These motions, whether mechanical or vital, will be sustained till one of these circumstances occurs,—till the diseased part admits a free passage, the blood finds an outlet from the carotid, or the heart's action is reduced. Unfortunately, in most instances, the mechanical influence of the impeded blood, or the nervous irritation which it induces, excites the heart to an increased action, and thus contributes to keep up the local irritation and disease.

The appearances admitted by Dr. Parry as constituting the changes in the arterial tubes,* and which are generally mistaken for contraction and dilatation, are locomotion during respiration, a longitudinal motion corresponding with the respiration, and a similar one with the diastole and systole of the heart, a lateral motion of the whole artery forwards and backwards, and longitudinal in both directions, a rotatory or curved motion in connection with the systole of the heart, dilatation from pressure, a retrograde motion in a divided artery corresponding with the systoles. That some of these effects, and therefore what is called a pulsation of the artery, should be made manifest on applying the

* Exp. Enq. p. p. 11, 13, 18, 30, 36.

torture of ammonia, or of any other stimulus, to the exposed vessel of a living animal, especially of a rabbit, dog, or cat, is so far from being doubted, that it would excite surprise if they were not produced; but how they can be reconciled with irritability, as a contraction on the application of a stimulus, is not very evident. The opposite state should, in fact, occur under such an influence. Neither is there any necessity for the intermediation of such a power under the increased action of the heart, its relatively altered condition, or the admitted changes which occur as causes of further varieties, in the common circumstances of examination and experiment. It is, indeed, supposed to be conclusive as to local irritability in its proper sense, that, on the application of ammonia to the aorta of a rabbit, the increase of motion did not extend to the part which was not under the direct influence of the irritation. This experiment has been repeated.* The aorta of a full grown buck rabbit was, to a considerable extent, exposed, by two intelligent surgeons. Not the slightest appearance of contraction and dilatation, nor even of motion, whether from respiration, or the increased struggles of the animal, was discoverable. The vessel was touched with Liquor Ammoniæ, and after a few minutes, during which no motion of any kind had appeared, a palpable dilatation occurred in the part which had been touched. The part, an inch in length, which was nearer the heart, at first remained as before, but seemed gradually to suffer

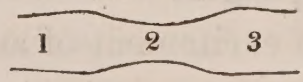
* P. 26—4.

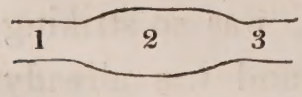
from the effects of exposure, so as at length to become, in its whole length, somewhat contracted. During several successive examinations, and applications of Ammonia, no movement was visible, except in connection with impulse or altered position, or in consequence of some locomotion, or the motion of neighbouring parts. The line of light on the artery remained an unchanged and conspicuous measure of position. No friction, not amounting to pressure, produced any effect. It was very clear to those present, that there was neither local nor general contraction, nor dilatation.

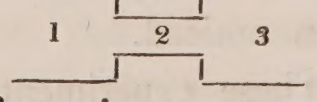
The artery was then detached, and by the application of Nitric Acid, a large portion evidently contracted itself longitudinally, and in its diameter. Liquor Ammoniæ and the point of the knife applied to another piece of the same vessel, had no similar effect. The œsophagus, stomach, heart, and intestines, under the excitement of any of these causes, exhibited their peculiar motions, in a very marked manner. The comparison between the effects on the œsophagus, and on the artery, was so striking as to excite particular admiration, and has already been noticed.

These experiments seem to detach any effects, which may appear in the living artery under exposure, from any vital agency similar to that of Irritability, and to throw the alternative of any contractile effort, either on the influence of altered temperature on the tunics, or, in some way, on the blood itself, diminished in quantity, or modified in its chemical

or hydraulic relations. We know that, under the influence of certain opposite conditions, as of irritation of various kinds, this fluid increases in quantity, and becomes altered in quality, and thus occasions a dilatation, not a contraction, of the vessels.

In describing the powers of Arterial Dilatation and Contraction, its advocates still leave us very much in the dark as to its nature; and though we may not venture yet to enquire, how such a process, occurring in tubes similar to the arterial tubes, bearing no analogy in their structure to that of the heart, could assist in circulating the blood, we may take the liberty to ask, what is the real phenomenon under examination? Does the arterial pulsation, so distinct to the sight of these experimentalists, consist of a set of successive portions of the tube alternately contracted and dilated, as might occur with a series of minute hearts? Is one part dilated while another is contracted, thus, 

and do the parts, as visible to the sight, constantly alternate, so that No. 1 shall then contract, and No. 2 dilate, thus, 

or is the form which they so plainly discover more like this figure, 

or, what is the exact local configuration, respecting which so much is affirmed? Is the whole set of arterial tubes dilated at the same moment, and contracted at once from the heart to the capillaries? Does this extended process alternate with the universal capillary or venous change? Or what is the real

state of the case? These are merely preliminary questions of information, upon the answers to which must, of course, depend the share which may be assigned to the vessels in assisting the circulation, or the determination of their peculiar function.

It will, also, be necessary to enquire much more minutely into the tortuous and vermicular motions ascribed to the irritability of arteries, and to ascertain the precise difference which exists between such motions, and the supposed contraction and dilatation of these vessels.

Before we can reason upon any presumed analogy in this case, we must obtain a much more accurate knowledge of this imaginary condition, a phenomenon, the existence of which, as hitherto described, the positive evidence of sight leads us entirely to deny, and the nature of which we must at present continue to explain in a very different manner.

A third ground, on which the irritability of the arteries is particularly maintained, is the Retrograde Motion of the Blood, under circumstances of obstructed circulation; a subject which has already been diffusely considered in the Additional Experiments. In this case, no direct evidence is adduced of contraction on the application of a stimulus; but it is assumed as a necessary change, from the fact, that, if the circulation can be carried on by connected anastomosing vessels, those which are obstructed become emptied of their contents. This fact is explicable under any contractile power of the vessel; if, indeed, it be a fact, that a contraction, which is not complete to the

closing of the tube, can entirely empty the vessel. The occurrence, however, is much more probable under the adopted tonic power, than under any known influence of irritability, whose faculty of contraction is, in this respect, far inferior in degree. The fact itself is one of those hypothetical cases, which, as far as relates to the arteries of warm-blooded animals, has been, in the Additional Experiments,* admitted for argument's sake, though not assumed by the author himself. It does not surely require much force of imagination, or extension of probability, to believe, that a contractile power, which can even permanently close the tube itself, may, for a time, when such artery is rendered unserviceable, contract on its contents, and expel them. But this is far from being the single alternative, which excludes the specific muscular irritability of arteries, as the cause of certain well-known phenomena, to which we shall shortly have occasion to allude at greater length.

Leaving the subject of arterial contraction, what are the supposed proofs of even a higher degree of irritability, which has been attributed to the Capillary System? The phenomena of this circulation are, chiefly, observed in animals with cold blood, and have, therefore, strictly speaking, no connection with any part of the subject as it has been treated by Dr. Parry. It is, however, not the less true, that many arguments, derived from these animals, are supposed to be conclusive against the doctrines and positions

* P. 108.

of this author. In one hundred experiments, conducted with his usual caution through a period of five months, Haller never could discover any contraction in the vessels of these animals. His contemporaries and successors never could discover any contraction, whether in the arteries, capillaries, or veins, of such animals. A late writer is satisfied that he has made the discovery. In these instances, taken from the capillary system, the boasted contraction and dilatation have no place. The ocular evidence of contraction on the application of a stimulus, and its presumed connection, as a cause, with the manifest effect of accelerated circulation, are the circumstances which are supposed to warrant the assumption of irritability as a property of this minute series of vessels. As far as relates to ocular demonstration, we must still remain at issue. Let us, however, first, examine the fact of a circulation locally accelerated on the application of stimuli; and secondly, the supposed proof that it arises from a contraction of the vessels, identical with this peculiar power. It unfortunately happens, that, in the experiments, so clear a distinction and separation of the vessels is not made, as the precise nature of the subject seems to require. It is, to say the least of it, difficult to determine the exact amount of effect in each order, where arteries, veins, and capillaries are all equally implicated, and alike affected by the exciting causes. In illustration of the points above mentioned, let us, however, enquire into the effects produced by external pressure, ob-

* Hastings, p. 48. † Pp. 52—3, 4, 5.

struction with a needle or ligature, or by excision of the heart. In the experiments already cited, these causes produced a slower motion in the blood, or else certain retrograde or oscillatory movements. Shall these effects be considered the proofs of a distinct power of circulation in arteries, veins, and capillaries? a diminished motion, or entirely altered state of the circulation, when the heart is removed, or its action interrupted? The slower motion will, certainly, not be so adduced; and to those, who have frequently observed the latter effects, and know their numberless alternatives, these motions will not be considered as supplying any proof at all. But what are the proofs of contraction in the capillaries on the application of a stimulus? In the experiment at p. 51, the *artery* was contracted, and the capillaries were dilated, while the blood in them moved slower; which dilatation, though the capillaries themselves had not been touched, continued through the whole experiment. So that excessive action in one vessel produced, as might be expected, dilatation of the vessels beyond it. What peculiar and predominant vital irritability does this experiment attach to the capillary system? or what similarity is manifest in the effects produced in the two systems? In another experiment,* all the vessels dilated, and the blood moved slower. Is this a proof of the contraction of the capillaries from the influence of stimuli? Does it shew any diversity in the three sets of vessels, or warrant the common assump-

* P. 54.

tion of a higher grade of this faculty in the capillary series? In the next recited experiment,* Salt dilated the three classes of vessels, the blood became red, and its motion slower. The application of Spirit of Wine, it is said, contracted the vessel, and quickened the circulation. Was any restoration of the blood to its natural state effected by this latter process? In the complex relation which is here established, it does not seem quite safe to draw any conclusion as to an exclusive agency on the tunics, or to determine that the resolution of the blood was not efficient in the subsequent return to a previous state. We do not, indeed, learn whether the effect was strictly local, or whether there was, in this experiment, any evidence of suffering in the animal.

Can we, however, believe it possible, that the web of a frog's foot should be immersed in water at 115°, without quickening the circulation, or that five similar immersions in water of that temperature should fail to occasion a change in the blood itself? These things may occur, without proving the irritability of the capillary tunics. Neither can their contraction, on the application of Ice,† be admitted as proof of their possessing this faculty. We have, indeed, one instance, in which ice, at first, contracted and quickened, and, after half an hour, dilated the vessels, and rendered the motion slower, which was then quickened and contracted by the application of the Oleum Terebinthinæ. It has already been observed, that as to any evidence of the contraction of

* P. 54. † P. 55.

the capillaries from inspection, we must still remain at issue; and admitting the occasional effect of increased velocity on the application of some powers, both sedative and stimulating, we may enquire, whether the irritability of the vessels is the cause of this really uncertain appearance. Independently of the common alternative in the modification of the heart's action, or the altered condition of the larger vessels, I cannot help ascribing much that may not be explained by the tonic or elastic powers of the minute vessels, to changes in the blood itself, so particularly manifest under the circumstances which accompany the phenomenon in question. While no minuteness of examination, with or without the lens, has enabled various observers to detect the supposed alteration in the vascular tunics, the changes in the blood itself, under the influence of stimuli, have escaped the observation of no single experimentalist. It is, certainly, true, that the share which is primarily to be allowed to the admitted inherent powers and affections of the vessels, and that which is to be placed to the account of changes in the state of the fluids themselves, has never been accurately distinguished, and constitutes the great difficulty of the question. To such changes in the blood, as an alternative, some of these appearances were ascribed in the Additional Experiments.* The vitality of this fluid, the physical cause of life, and the changes consequent upon the application of stimuli, in its vital or chemical relations, seem, indeed, after fre-

* Add. Exp. p. 192.

quent observation, to be so much greater in point of degree, and so much more important in point of quality, than those of the tube which is the mere vehicle or "carrier," that, in the absence of direct evidence, it is perhaps more consistent with probability, to ascribe to these causes some of the conspicuous effects; and among others, the accelerated motion through the vessels, under certain circumstances of irritation. Stimuli appear in this respect to act on this important vital fluid through dense media, very much as the magnetic influence operates upon its related properties, and to excite motion in matter with which it has affinity, even through obstructing, or intervening, substances. It is evident, that, under the impulse given by the heart, the acceleration, with some exceptions, can have but one direction, that of the natural course of the circulation. To determine what influence particular stimuli may have in rarefying the fluid to be circulated, or in producing a tendency to a vacuum, and thus facilitating the motion of the fluid, is not within the reach of our present knowledge. We shall shortly have occasion to attend to a theory, which imagines an absence of the attraction of cohesion to be the source of all the regulated and accelerated motion of the blood. In the case immediately before us, it is evident that neither the capillary motion, which, after adoption, Haller ultimately discarded, nor the capillary attraction which has recently been proposed, can have any influence in the production of the phenomena; since the attraction

should rather be from the veins, and in the capillaries, a repulsive power should rather be exercised.

In frequent inspection of the capillary circulation, so far is it from having appeared superior, in its capacity of being affected, to the venous and arterial systems, that, under the irritation and excitement of the whole web, it has more frequently happened, that there was an immediate or early suspension of motion in this series, than in any other. This quiescence has ocurred in numberless instances, while motion was proceeding in the veins and arteries. It appears, indeed, from the experiments of certain authors, who, nevertheless, maintain the contrary doctrine,* that the capillaries have an inferior, instead of a greater power of vital contraction than the other orders of vessels; since, though increased contraction be the sole source of increased local velocity, circulation is according to them, slower in these vessels, than in the veins, or arteries; and since, upon the same authority,† the former vessels are only affected equally with both these sets, by the application of stimuli. Comparing it with the circulation in the veins, and taking into account the effect of diminished capacity of tubes, which, notwithstanding the ingenious arguments of certain philosophers, should, in this case, as in others, have the usual result of accelerated motion, the rapidity of the circulation in the capillaries can, it is evident, furnish no argument against the sole or paramount agency

* Hastings, p. 41. † P. 47.

of the heart, in this important, and still mysterious, process.

That no argument can be conclusive in favour of the vital irritability of the vessels, whether arteries, veins, or capillaries, or against the tonicity of Dr. Parry, from the motions called retrograde or oscillatory, has been shewn in a former essay, and has already been noticed here. We might, *à priori*, venture to ask what argument in proof of the supposed irritability of the vessels can be drawn from the fact, that, when the heart's influence is withdrawn, the circulation does not proceed with its accustomed regularity? It seems difficult to imagine that an irritability, of which the palpable effect, in this much cited argument, is to produce a state the reverse of that which is the natural one, by which, as Haller expresses himself, nature would have armed the vessels for the destruction of the circulation, should be claimed as their necessary and peculiar property.

That the continued circulation after death, and the occasional emptiness of the larger arteries, in warm-blooded animals, receive, according to Dr. Parry's opinion, a ready explanation from the gradual contraction of the larger arteries, meets no kind of refutation from the circumstance, that in cold-blooded animals, when the heart is removed, or a ligature is placed on the limb, so as to intercept the heart's influence, there may be an oscillation in the arteries, and a retrogression in the veins. The case does not apply to Dr. Parry's doctrine, and, under the

admitted theory, seems to have no bearing upon the question of the irritability of the vessels, since such a deviation from the natural action, or course, on suspension of the heart's influence, might as readily occur in glass tubes, under the alternatives suggested by an examination of the phenomena themselves. Under the altered condition of the blood, when the primary cause of motion is suspended, it is natural to suppose, as appears to be the case on inspection, that some hydraulic and chemical changes occur; and though, when, in warm-blooded animals, the regular circulation is, for a time, continued, the tonic contraction of arteries may be assistant towards the expulsion, yet in cold-blooded animals, in which even this tonic contractility is a matter of doubt, such changes seem to have a decided share in producing the phenomena. It is impossible not to feel a persuasion, that, under these circumstances, gravitation, attraction, repulsion, a modification of some expansive or elastic agencies, the influence of cold and of heat on the blood, the operation of certain stimuli, particularly the predominating force of currents, are more concerned in the production of the effects, than any dilatation and contraction, or even than the more simple, but in these cases equivocal, power of contraction itself. The influence of gravitation alone is so decisive, as to render the choice of a retrograde or oscillatory motion, subject to the controul of the examiner, and the influence of currents in modifying the alternating and varying movements, is so little questionable, as, of itself

alone, to supersede any exclusive agency which might be attributed to the supposed irritability of the vessels. The repeated observation of Haller and of Whytt, to which we shall again have occasion to allude, that the same movements occur in blood effused between the membranes of the mesentery; and the fact, that they occur in vessels almost emptied of their blood, "mira oscillatio, cum tamen pene inanis esset arteria;" are themselves sufficient to disprove the supposed origin of these movements.

Notwithstanding all that has been said, in confirmation of an analogy between the system of blood vessels, and parts decidedly irritable; and notwithstanding the alleged grounds of the manifest differences, which exist in the effect of stimuli in the two cases; we are strongly disposed, from this very diversity itself, to deny the similarity between the arterial powers, and those of such other parts. In those parts, when the application of a stimulus does not very early produce the effect of contraction, such effect is either not produced at all, or if produced, is attributable to another cause. It is certainly asking too largely, to expect that we should wait one or two hours for an effect in arteries, which we can immediately produce in other parts; and that we should assign it to a cause, which is, at best, only coincident or concurrent with others, whose character and influence is much less equivocal. As we altogether deny the fact of contraction and dilatation in the arteries, that most important analogy which has been assumed between these vessels and the heart,

cannot easily be admitted. While, in the latter, such an alternating state does probably occur, as an automatic and constant motion, and which, as it is strongly marked when the veins are tied, is independent of impulse; in the arteries, its occurrence is altogether denied under intercepted impulse, and might, in itself, easily be explained, without the interference of vital irritability, by the circumstances in which the arteries are placed with regard to the heart. The effects of stimuli on the heart deprived of its blood are very striking; and if the degree of irritability is to be estimated by perpetuity of function; if, as Haller decides, those parts which "perpetim agunt," are most irritable; and if, on this principle, the heart is classed as the most irritable of all the organs of the body; why should not the analogy be perfect, in this instance also, between the heart and the arteries, to which a similar function is attributed? Instead of any doubt as to the existence of this faculty, it should be as evident in the extensive vascular system, as in the primary organ. The reverse, however, actually happens. Separate an artery from the body, touch it externally or internally with the point of a knife, or blow air into it; what effects are produced? Absolutely none. Whereas, under the influence of such irritation, the heart resumes and continues its natural processes for an indefinite length of time, and ultimately remains more dilated than during life. The arterial contraction after death is greater than in life, and continues sometimes for twenty-four hours without any dilatation, which never ultimately amounts

to the degree existing during life. The arterial state is such as would be conspicuous, if the vessels were deprived of blood[1]; that of the heart, as if this organ were more than usually supplied with its natural fluid. Supposed parity of function should imply parity of action; but the analogy fails altogether, in this respect, between the heart and arteries. They manifest a conformity of effect, neither under their natural processes, nor under the influence of artificial stimulation.

Reference has already been made to the effects produced by the point of a knife on the heart, intestines, stomach, œsophagus, and the entire dissimilarity of state in an artery under the same circumstances. In excuse for such inaction it will scarcely be urged, with much perseverance, that the same stimuli will not equally affect all parts. Where, as has been shewn, for perpetuity of supposed function, there should be a high rate of this power, it might be much more consistent with probability to believe, first, that the arteries should also undergo that appropriate action, which other irritable parts manifest on the application of a stimulus, than that this should be entirely wanting; and secondly, that, if this effect be wanting, the natural power is possibly devoid of resemblance to that possessed by irritable parts. That cold and exposure will produce in all parts, whether irritable or otherwise, a contractile effect, which will be somewhat varied according to diversity of structure, will not be denied. It has, however, before been observed, that the resemblance between the effects produced by these causes on the

intestinal and arterial tubes are too remote to warrant any conclusion as to an identity of properties. Neither can any analogy be admitted between the thrilling or quivering movements of muscular fibres, when irritated, and the appearances induced by similar causes in the vascular canals.

We have hitherto been engaged in the consideration of this subject, in its connection with experimental enquiries. The present occasion will not permit any very extended observations on this point, nor any examination of the phenomena of the venous system. It remains, however, to pay attention to certain arguments, in favour of the arterial irritability, which are of a more speculative kind, and which have been advanced, with much confidence, since the publication of the works already noticed. Some general remarks will be reserved till after the examination of these hypothetical doctrines.

In an Essay on the forces which circulate the blood, Mr. Charles Bell adopts many doctrines opposed to those of Dr. Parry. He contends that the arteries must be irritable, because it would be impossible for the heart to propel the blood through the arteries and veins of different calibers and curves with the same force, or with any well regulated impulse; and, because, the force of the heart cannot account for the irregularities which we observe in the force of the arterial action.* These opposite, and antithetical propositions, do not appear to furnish any very safe or consistent argument, in proof of the

* Essay, pp. 32, 40, 81.

question at issue. It seems possible that, if the heart cannot propel the blood with the same force through all the vessels, it may, by its circumstances, explain the irregularity of the force with which this fluid is actually propelled; but by whom has the heart ever been so charged with all the local varieties of circulation? If it be not quite clear, that local enlargement of the arteries, and its consequences, may not, on some occasions, be ascribed to the force of the heart, where has their contraction been attributed to the influence of this organ?* Although it were admitted that the force of the heart is sufficient to carry the blood through the whole extent of the Arterial System, "it cannot," says Mr. Bell, "be "made to account for the inequalities which we "observe in the arterial actions. It cannot account "for increase and diminution of secretion; for sud- "den and partial growth; for wasting and decay of "parts. Ought not these facts to be accounted for?"† Surely, Mr. Bell has himself philosophically accounted for some of these occurrences, without any necessity for the interference of the supposed faculty of Irritability, or indeed of any other vital power of the vessels. He has satisfactorily proved, that their structure explains the inequalities observable in the different parts of the system. "The tree of the "arteries presents the most simple explanation of the "forces circulating the blood. Here we see branches "of different lengths and curves, and which, there- "fore, receive the blood from the heart with differ-

* Essay, p. 38. † P. 32.

"ent degrees of force."* Mr. Whytt, too, proves that, independently of any assistance from a vital power, "the velocity of the blood will not be the "same in all the arteries even of the same diameter, "but will be greater or less according to their dis-"tance from the heart, the excess of the areas of the "branches above their trunks, the angles at which "they go off, and the number and degree of their "flexures."† We learn, also, from Mr. Hunter, that "to keep up a velocity sufficient for the parts, "and no more, nature has varied the angle of the "origin of arteries, at different distances from the "source of circulation."‡ But what have the rest of Mr. Bell's positions to do with the question of the circulation? Is it intended to prove that this cannot proceed without the arteries, because the heart is not able to perform various functions with which it can have no possible concern? Has any one denied the complexity, or local origin, of the phenomena enumerated, or attempted to ascribe them to the heart? The argument for the necessity of arterial irritability, drawn from the circumstance that in some animals the heart is altogether wanting, in others accidentally deficient or extensively diseased, is so devoid of novelty, has been so often completely refuted, and the converse of the proposition has been so ably maintained by Haller and others,‖ that we may be permitted to proceed at once to the consideration of

* P. 66. † P. 217. ‡ P. 163.

‖ Haller Phys. vol. i. pp. 422, 438, 505. Whytt, p. 229. Cuvier Leçons d' Anat. Compar. tom. iv. p. 178. Elem. of Path. and Ther. p. 20. Additional Exp.

the direct arguments that have been derived from the facts of the arterial system itself.

Mr. Bell supposes that the arteries are irritable, and perform all the functions above assigned to them, because, in the first place, they at one time diminish, and at another increase, the velocity of blood. To say nothing of the petitio principii which this argument involves, and of the want of proof that the assumed vital power can alone produce the specified effects, Mr. Bell has himself shewn, in the passage already quoted, that such effects may be the consequence of the admitted hydraulic conditions of the vascular system, and that they have therefore no necessary connection with any properties of a vital kind.

In regard to the examples of such increased velocity, what will the advocates for debility and stagnation, say to the proof of irritability drawn from the increased velocity of the blood in an inflamed part? "If we cut into an inflamed part," says Mr. Bell, "the blood flows with more velocity than in the "natural state of the body."* It will be remembered, that the doctrines of Dr. Parry do not necessarily imply an increase of velocity; on the contrary, the cases adduced, as illustrative of the state of inflammation, suppose a local velocity as great only as in the corresponding healthy vessels.†

Mr. Bell argues, secondly, that the arteries are irritable, because Mr. Hunter proves them to possess a living power of contraction; a fact which is also proved by every author who writes on this subject.

* P. 54. † Elem. p. 63.

But, thirdly, this power is Irritability, because, first, anatomy shews the middle tunic to be muscular, and secondly, because the experiments of Dr. Hales demonstrate a contraction on stimulus. First, as to the muscularity of arteries. "I read," says he,* "in a "French work, where, with the utmost indifference to "all we know through the labours of Haller, Whytt, "and Hunter, it is written that there are no muscular "fibres in the arteries. The author speaks with a soft "indulgent regret of those who have been so long "deceived into a belief, that arteries have muscular "or irritable fibres." And again,† "against the "evidence of human anatomy, against the analogy "of the structure of inferior animals, against the "direct evidence of the senses, and on the mere "ground that some chemist has thought he has dis-"covered that there is no fibrine in the texture of "arteries, an opinion is taken up that the arteries "are not muscular. It really appears as if men were "desirous of altogether disavowing and overturning "the labours of the great men of our own country, "who have gone before them. I cannot imagine "men possessed of any relish for philosophical en-"quiry, or capable of pleasure in witnessing the "exertion of genius, thus disregarding the labours "of Mr. Hunter upon this subject."‡ Dr. Parry speaks§ of the want of muscular power in the great arterial trunks, as a fact assumed by the more modern anatomists; but himself admits the existence of

* P. 36. † P. 53. ‡ P. 54.

§ Elem. pp. 14, 15, 25.

some weak muscular fibres, particularly at the root of the aorta. In his inferred want of analogy between the structure of the arterial middle tunic, and that of muscles, he may, perhaps, still be considered as safe in his dependence on the philosophical accuracy of Berzelius, as on the amiable scepticism of Mr. C. Bell, were there even no sufficient authority in our own country to support his hypothetical argument. But is it clear that the authorities, on which Mr. Bell relies as sacred and inviolable, go to the extent he asserts, and carrying the demonstrable muscularity through the vascular system, that they warrant the inferences which he draws from their supposed doctrines. Haller, indeed, says, "Fibræ carneæ quas in arteriæ "aortæ principio imprimis contemplatus sum, in mino-"ribus arteriis sensim malignius adparent, in capil-"laribus denique nec lente quidem vitreâ adhibitâ, "qua rerum species augetur. In animalibus frigidi "sanguinis nihil quidquam earum fibrarum simile in "plurimis experimentis reperi."* "Quæ viva vis "arteriis inest, ea pertinet ad fibras musculosas, haud "multas, haud valde sensiles, quæ in arteriis majoribus "calidorum animalium reperiuntur, cæterum naturâ "irritabili debillimâ gaudent."† "Non est cur "*omnino vitalem vim contractilem* arteriis deneges."‡ All his examples of the vis contractilis, to which Haller scarcely dares to assign the name Irritability,§ are

* Op. Min. vol. i. p. 377. Phys. vol. i. p. 63.

† P. 64. ‡ P. 441.

§ For various authorities as to the Irritability of Arteries, see Additional Exp.

taken from the Aorta, or derived from the experiments of other authors, the accuracy of some of which he is not willing to admit. After denying the influence of stimuli in producing contraction of the tunics, he founds his cautious and qualified opinion, "non est omnino," on the single fact, that, when tied, the Aorta empties itself of blood.

Whytt has no experiments to prove the muscularity of arteries. He assumes the fact, and with a single exception, relies wholly upon preceding authorities. "It is," says he, "generally allowed that the "systole of the larger arteries, in which there is a "remarkable pulsation, is owing not only to the "elasticity of those vessels, whereby they endeavour "simply to recover themselves, but *partly* also to a "*proper muscular* contraction of their tendineo-"carneous coat: and, as this is excited by the blood "thrown into them by the heart, which, at the same "time that it distends their fibres, gently irritates "their internal surface, it seems *reasonable to allow* "that the smaller vessels, endowed at least with equal "sensibility, must be excited into feeble, but conti-"nually repeated, contractions, by the gentle stimulus "of their circulating fluids."* According to Whytt, the diminution of the strength of the pulse in an arm that is quite palsied, is a strong proof that the larger arteries act partly by a muscular power. So that he has not, like Mr. C. Bell, any reliance on anatomy, and draws his proof from a position, which, in the first place, has no foundation in fact, and, in the second

* Whytt's Works, 4to. p. 229.

place, would, upon his own principles, serve as no proof at all; since the sensible pulse being the supposed diastole of the arteries, and this depending entirely on "the blood pushed into the arteries "with a considerable force,"* the relation should be between the heart and the supposed effect, and not between that effect and the arterial tubes. But Whytt tells us himself, that he only concludes the small arteries to be in some degree muscular, from their power of motion in living animals; and as he ascribes to the laminæ of the mesentery precisely the same effects as to the small arteries, it must appear that he had no certain conceptions as to the real nature of this structure.† "It has been observed "that a few globules of blood, extravasated between the "laminæ of the mesentery, ascended and descended it "regularly, and were agitated with the same kind "of oscillatory motion, as in the small arteries."‡ This is a very extraordinary admission; since, on the other hand, it extends to these laminæ all the powers of muscularity and irritability, which the argument deduces in favour of the arteries themselves. Whytt trusted to authority, and his own preconceptions of the necessity of a muscular structure, from the functions imaginarily assigned to the arterial and vascular system.

But is Hunter, on whom Mr. Bell entirely relies for the security of his opinion, more decisive on the subject of muscularity, as a necessary constituent of this system? "The muscular power,"‖ he observes, "has by some been overlooked, by

* P. 53. † P. 234. ‡ P. 235, ‖ Hunter, p. 113.

"others denied, and has only been asserted by others "as appearing necessary by reasoning from analogy." His own proof of muscularity is so far from being taken from the direct evidence of anatomy and the senses, that it is drawn, first, from a comparison of the action of arteries with that of elastic substances, and, secondly, from the various assumed uses of these organs. "When the various uses of arteries are "considered, we cannot but conclude that they are "possessed of muscular powers." Again—"parts "endowed with considerable elastic powers, although "*not apparently* muscular, as many arteries, we yet "know, from other modes of information, to be "possessed of muscular power." "When we con- "sider the construction and use of the arteries, we "must at once see the necessity of their having "those two muscular and elastic powers; yet, in the "greatest number, it is impossible to give clear "ocular demonstration of the existence of distinct "muscular fibres; but still it is reasonable to conclude "that they exist."* Again—"every part of the "vascular system is not equally furnished with mus- "cular fibres; some parts being almost wholly com- "posed of the *elastic* substance, such as the *larger* "vessels, especially the arteries, in which, were they "equally muscular with the smaller vessels, the ex- "istence of muscular fibres might be more easily "proved. From my observations and experiments, "I have discovered that the larger arteries possess "little muscular powers, but as they recede from the

* Pp. 114, 115, 117, 118.

"heart towards the extremities, the muscular power "is gradually increased, and the elastic diminished." From these quotations it appears, that Mr. C. Bell's exposition of the doctrine of Mr. Hunter is very inaccurate, and it is not a little curious to observe the essential difference which exists between Haller, Whytt, and Hunter, on a subject, in which they are cited as concurring authorities. Instead of the evidence of anatomy and the senses, Mr. Hunter disclaims them altogether, as incompetent and uninstructive on this point, especially in the situations in which, according to some other anatomists, and physiologists, the application of these tests is admitted. He relies wholly on theory and probability for the support of his doctrine; and believes this structure to reside chiefly in seats, where he denies the possibility of discovering it. So decisive, indeed, is Mr. Hunter on this subject, that he elsewhere repeats his ignorance of these fibres, and asserts his belief that they are not such as they have been described by preceding authors, and that, as so described, they are incapable of producing the effects which have been assigned to them. "What the direction of the "muscular fibres may be,* *I never could discover*, "but should suppose them oblique, because the "degree of contraction appears greater than a "straight muscle could produce, in which light a "circular muscle is to be considered, as its effects are "in the direction of its fibres." After this view of Mr. Hunter's real sentiments, it is scarcely neces-

* P. 121.

sary to pursue this argument any farther. On the authority of this great man, neither the larger, nor the smaller arteries furnish any sensible evidence of muscularity; and if we may be permitted to charge him with an error, it is that of imagining only one sort of vitality, and of hypothetically assuming muscularity as the only living power competent to the production of certain effects, to which it seems by no means necessary, and with which, indeed, it appears wholly unconnected as a cause.*

The opinions of the admirable Bichât, in relation to this subject, are well known. He asks, "Quelle est la "nature de la fibre arterielle? Presque tous les "anatomistes la croient identique à la musculaire. "Mais pour peu qu' on examine attentivement les "objets, il est facile de se convaincre de leur differ- "ences."† Dr. De Felici, of Pavia, one of the latest foreign authorities on this subject, thus expresses himself:‡ "Le fibre di questa membrana "media formata a strati circolari i di cui anelli diretti "un poco obliquamente non sono mai compiti, "differiscono evidemmente dalle fibre muscolari or- "dinarie per il loro colore gialloguolo, o biancastro, "et per la loro figura appianata." He even asserts, "La pretesa azione contrattile delle arterie "e contradetta dall' esperienza.‖ Il celebre Prof.

* See pp. 114—15. † An. Gen. ii. 281.

‡ Saggio sopra la Circolazione del Sangue e sull' Origine del Polso, del Dottore G. M. De Felici, Pavia, 1818.

‖ Caldani Inst. Phys. § 120. Jacopi Elem. di Fis. e nat. comp. P. i. p. 210.

"Caldani dopo di avere esposti molti argomenti "che gli sominstravono le piu recenti osservazioni "cosi conchiude. Non igitur ab arteriarum sese "contrahentium vi, sed a systole cordis sanguis in "venas promovetur. Si e osservato che tagliando de' "grossi tronchi arteriosi, il loro lume aperto tosto in "qualche modo si ristringe : ma cio non provarebbe "che l'elasticita, e la contrattilita delle arterie che e "commune al' tessuto cellulare, al peritoneo, &c." The great stress which Mr. C. Bell has laid on the opinions of the authors first recited, made it necessary to pay some attention to his argument drawn from this source; and, however little we may be disposed to sympathize with him in the sensibility with which he professes to contemplate any secession from the dicta of those who have gone before us, we can readily admit that much respect is due to the authority of great names, whether of ancient or modern times. The labours and opinions of those who have worked, or are still engaged in the same field of enquiry, are, in fact, the steps by which science attempts to ascend its still distant summit. It might, however, not be very difficult or irrelevant to shew, with regard to this author himself, that, notwithstanding all he has said on this subject, and the little respect he manifests for his contemporaries, whether they do or do not venture to think for themselves, he has a singular reserve for his own speculations, and that his practice, upon many occasions, differs strangely from his precepts and professions.

To return to the muscularity of the arterial tunics. It is scarcely necessary to intimate, that the argument is of no avail, which maintains, that in parts decidedly irritable, and, therefore, muscular, the fibres differ materially one from the other, unless you prove at the same time, what we deny, the strictly irritable nature of the vascular system. But what is Mr. Bell's own proof of muscularity? He first assumes, that irritability, a property of muscularity, has for its vital effect contraction, and a sudden and complete relaxation; and then proves the existence of muscularity from the supposed effects of this power in the following instance. The aorta taken out of the body of a turtle was so completely *contracted*, even *while the heart continued to beat*, that no force could inject it, neither could the desired effect, a necessary vital effect, be produced during the life of the parts, but only when these became relaxed in death.* This is a very singular argument, and a very singular mode of proving the premises.

Mr. Bell's second evidence of the irritability of the arteries, and their assistance in the function of circulation, is derived from the well known experiments of Dr. Hales, very ingeniously devised, like every thing that proceeded from this acute philosopher, and by him adduced to illustrate the relaxing qualities of warm water, and the styptic effects of cold water, brandy, and other astringents. on the coats of vessels in the animal killed by bleeding to death.† "Physiologists," says Mr. C. Bell,‡ "have

* Essay, p. 35. † P. 133. ‡ Essay, p. 33.

"not enough considered, that muscles will sometimes "resist all stimuli, but that of their proper agents. "How, then, shall we contrive to excite the arteries, "in imitation of their natural stimulus? Or, suppos- "ing that we succeed in causing them to contract, "how shall we perceive the action? Dr. Hales "found that a certain quantity of water passed from "the cut vessels in forty-six seconds, while the same "quantity of brandy took sixty-eight seconds, and "on a second trial seventy-two seconds. He found, "by the same mode of experimenting, that warm "water, of the temperature of the animal, passed off "much quicker than cold water from the pump. "The natural inference from this experiment is, that "the cold water and the spirits excited the muscular "fibres to diminish the calibres of the vessels, and "hindered the course of the fluid."* First of all, then, according to Mr. C. Bell, warm water is a stimulus analogous to the natural one. What is its effect? A continued relaxation of the tunics, suffering the entrance and transmission of fluids. Secondly, cold water and brandy are not analogous to the stimulus of the blood. What are their effects? Irritation of the muscular tunic, and the usual consequence of such excitement, a permanent contraction of the artery, and a greater or less interruption to the passage of fluids. So that, in succeeding to induce a contraction of the arteries, and to perceive this contraction, we discover an effect wholly opposed to the condition assumed, under the

* P. 34–5.

operation of what is called the natural stimulus, and an effect which impedes the circulation of the blood. In addition, then, to Mr. Bell's natural inference, we may satisfactorily conclude, either that the vital arterial power is not irritability, since this faculty impedes, rather than assists, the circulation; or, that whatever may be the real influence of the blood, or the properties of the containing tunics, the natural state is not one of excitement and action; or, that the experiments in question prove nothing as to the disputed points. But supposing that irritability does impede the circulation, by producing a permanent contraction, as is proved by these experiments, is it clear that, in the present instance, the effect is on the muscular fibres, or that there is no difference between this effect, and the known effects of irritability. A frequent repetition of these delicate trials must take place, before the fact itself is placed beyond all doubt. It may perhaps be then time enough to enquire, whether no other living power, besides irritability, can be productive of the varieties in question; or whether no analogy can be found between them and the effects which might be produced on dead animal matter; that is, whether permanent states of relaxation and contraction, as separate effects, can result from the operations of life alone.

Proceeding with his arguments for the irritability of arteries, Mr. Bell observes, "Because the relaxation "of the heart is quick and complete in proportion "to its natural and lively contractions, we *must* "*imagine* the same thing to take place in the artery.*

* Essay, p. 56.

"The excitement of a hollow muscle does not consist "merely in contraction, but in the free and perfect "relaxation which follows the contraction.* When "an artery is excited, it is not only excited to rapid "and universal contraction, but to sudden and com- "plete relaxation in the intervals of that action."

With regard to the first hypothesis, which Mr. Bell describes as a mere matter of imagination, he has indeed, in another place,† with much pains and emphasis, by arithmetic and argument, endeavoured to prove, in opposition to Whytt, Haller, and Hunter, and many other eminent writers, that though the state of arterial dilatation and contraction necessarily exist, it is utterly impossible, that it should ever be discovered by any sensible evidence. "Is it "reasonable," he asks, "to expect, that a lateral "motion of the artery should be observed?" And, with regard to the next positions, surely Mr. Bell has forgotten his own "natural inference" from Dr. Hales' experiments, in which the only effect of exciting the hollow arterial muscle was to produce a contraction, and impede the course of the fluid. But are the bladder and the stomach, in his opinion, hollow muscles? They both contract, on external as well as internal irritation, without any exhibition of a sudden and complete relaxation. The composite structure of these organs furnishes, it may be added, a much more perfect analogy with that of the arteries, than the structure of the arteries does with that of the heart. Mr. Bell appears, also, to lose from sight

*P. 58. † P. 30.

his own analogous case, the important instance of the turtle, in which death alone could produce this sudden and complete relaxation, even under the potent stimulus of an injecting syringe. The action of hollow muscles is not, then, as he describes it, an active state of dilatation and contraction. But what says Mr. Hunter on this point? More consistently than some other authors, he shews that "muscles "have only one action, contraction;"* that "the "contracted state of an artery arises from the action "of the muscular power, and is again restored to "the natural state by the elastic;"† a very different position from that of Mr. Bell, and assigning the relaxation, which may occur, to a very different cause from the supposed irritability of a hollow muscle.

Another argument in favour of the irritability of the arteries is taken from a different explanation, which Mr. Bell offers, from that usually given, of the fact, that the foot, with a weight on it, is moved at each systole of the ventricle. "When one leg is "twisted over the other knee, and the foot is held "suspended, a distinct pulsation is perceived in the "toe, and the toe will pulsate, and rise, although the "foot be loaded with a hundred pounds.‡ If the "knee be supported on a small pad betwixt the "hamstrings, or if the knee be suspended by grasping "the condyles of the femur, there will be no pul-"satory motion of the foot, although the foot hangs "at right angles with the thigh. Further trials will

* Pp. 101, 106, 109. † P. 117.

‡ Pp. 68, 69, 70.

"shew that the fleshy bellies of the muscles of the "calf, or ham, must rest upon the knee, to produce "the effect contemplated. It follows, that we are "wrong in supposing the motion of the foot is owing "to the popliteal artery changing from the curved "towards the straight line; or, in supposing, that it "is the attempt of the current of blood to force "itself against the compressed artery.* It is not "the popliteal artery which throws up the limb; it "is the pulsation of the smaller arteries, within the "fleshy substance of the limb, that produces the "motion." Whatever difficulties may attend this case, it is not very evident, what advantage is gained to the cause of Irritability, even by admitting the fact as here described; for the power of the smaller arteries might, in this instance also, be the same as that of the popliteal artery, under the view commonly taken. There is no evidence, that, without a pressure and intercepted current, and a consequent effort at "redressement," the effect would be produced in them; and, if it were true, that all the smaller branches in the belly of the muscle did produce this effect, no proof is hence derivable of a superior individual power, nor of any result, which might not, á fortiori, be expected from the combination of such forces, even beyond the effect which a large single artery might be capable of manifesting. What is gained by the cause, if the combined influence of these branches be superior, or if, under the circumstances described, the natural action be exhibited in

* Senac maintained the latter opinion: vol. ii. p. 49, 50.

a greater degree? or what proof is there as to the real nature of that action, except, perhaps, that irritability would not so readily produce the specified effect, as elasticity, or other mechanical agencies? It may be added, that, in some of the instances adduced in proof of the absence of pulsation, there is the decided failure, that they furnish no parallelism. No compression takes place, when the knee is suspended by grasping the condyles of the femur, or when the arm is merely bent; a condition, which, under the circumstances of the animal constitution, does not imply an interruption to the circulation.

The principal novelties in Mr. C. Bell's doctrines are, first, the hypothesis, that, in order to explain the facts of the circulation, and get rid of the resistance occurring under the common laws and operations of matter, it is necessary that "the attraction of "cohesion should be dismissed;" and, secondly, that tortuous arteries do not retard, but accelerate, the blood's force, by increasing the capacity or area, and, therefore, the influence and power, of these vessels. The former is a mere theory, which cannot, for the present, be admitted, however eagerly we may adopt any explanation, founded even on remote, and almost imperceptible, analogies, in a case so replete with difficulty as the laws of the circulation. On this subject, the following brief remarks may for the present suffice. As Mr. Bell admits,* that, without some such principle as attraction, "there could not "be any motion; and that, without it, motion would

* P. 12.

"soon perish;" the influence which the vessels exert on the blood, if any, however mediately induced through the exertion of vital properties, cannot be otherwise than of a mechanical nature. If then, as he asserts, contraction produce the motion of the blood, attraction must exist, or impulse could not have place. In such a case, how can Mr. Bell's negative attraction occur in the circulation, and at the same time his account be correct, as to the predominant arterial influence? The relation between the sides of the vessel and the fluid must be the same as in all other matter, and it is not only true, as he says, that, under the absence of this property, a smaller force would produce the effect, but equally so that no force at all can be exerted, in producing the effect of motion, under the condition which his theory implies. But what is the supposed parallel case? That as, in secretion, the chemical affinities of the blood are changed by the influence of life in the smaller vessels, and as that which modifies attraction may discharge it;* therefore, we may safely conclude, that the attraction between the fluids and solids is destroyed by the acts of life, in the process of circulation. In the first place, however, we may remark, that there is no analogy between circulation and secretion, which will admit any safe inference to be drawn from the latter to the former. Secondly, the process of secretion, itself a vital process, is no more a modification of the power of attraction, than any ordinary and simple chemical process of altered

* P. 13.

or deranged affinities can be so called. The new product is a total of preponderating relations; but is no change in the powers constituting the relation itself, by which the natural result is effected. Instead of any modification of the power, there is only a new result of the combination of old powers. Potass, Carbonic Acid, and Citric Acid, are severally endowed with a capacity of attraction for different kinds of matter. As the attraction of potass for citric acid, or v. v., is stronger than that of potass for carbonic acid, or v. v., the usual consequences follow; but here is no modification of the *power* of attraction, the natural constitution of the individual matter remains as before. The new product is a mere exhibition of the peculiar tendencies of the inherent power, which, under all possible circumstances, will, of necessity, become manifest. The modification is not of the power by which the product is formed, but of the power which this can exert in its subsequent combinations. That the affinities remain just the same as before, is clear, from the fact that we can predict the effects of other combinations, and the nature of the new products which will result from the developement of the acknowledged powers. The instances which Mr. Bell adduces, of the modification of the power of attraction in dead matter, an analogy which he extends to the vital process of secretion, are the difference which heat produces in the running of fluids through tubes, the modification of attraction which electricity produces betwixt fluids and solids, the different degrees

of attraction of water for glass, metal, or wood, and the repulsion of mercury by glass or iron. Does an accurate and minute analysis shew any real difference between these examples, and all other instances of the effects of interchanged affinities? Probably none. They are all varieties in the results of powers, but not modifications of the powers themselves.

The force of the arguments on the second head, the influence of tortuous arteries, depends on the assumption of numberless questionable facts and principles; and the subject can only be discussed here in its connection with the existence, and assumed uses, of the faculty of irritability. The doctrine assumes, in the first place, the hypothesis of negative attraction; under any other view, the curved form must necessarily retard, and not accelerate the circulation. It assumes, in the second place, the existence of muscularity and irritability, and their influence on increased velocity. It takes for granted, in the third place, that increased velocity is necessary to all the functions of parts, in which the specified changes occur. All of these points are disputable, and some of them, probably, the very reverse of the actual case. The first hypothesis has already been sufficiently considered. The facts of the second division we deny; and, from this author's own shewing, maintain, that they are not only unnecessary to the assumed end, but that the supposed effect is disproved, by the very experiments which are adduced in proof. The third assumption is contrary to all probability, and, in the examples cited, we can dis-

cover no evidence that such increased velocity exists, whatever may appear in favour of a larger amount of the fluid, for particular purposes, derived to particular seats. Such a structure cannot, Mr. Bell thinks, be for the retardation of the blood, because it occurs wherever the part is subject to an occasional increase of activity. "To consider the curve as "design, and that the intention is to retard the "course of the blood, instead of increasing the force "of the arteries, would be to suppose that mecha-"nical obstructions were interposed, where, in fact, "we find that there is a remarkable increase in the "force of circulation, and a growth of the part rapid "beyond example."* In these observations, to use Mr. Bell's own words, there appears "a very great "confusion" of matters. The presumption that increased *activity* must consist in increased rapidity, seems to be founded on a want of distinction, frequently admitted, between the purposes of mere circulation, and those of secretion and other functions. It is certainly not evident, that, in these cases, an increased activity implies an increased, or more rapid, circulation; and while it is not clear that this remarkable increase in the force of the circulation really has place, it is, at the same time, not evident that such a condition might not, if required, be consistent even with such a retardation as is usually attributed to the tortuous form. There might be such a coincidence. It seems, however, most probable, that, when the purposes of mere circulation are not to be

* P. 53.

answered, admitting that the demand for more blood is facilitated by such a structure, an increased velocity, and even a continuance of the same degree of velocity, would be less favourable to the purposes of secretion and growth than a slower process.

According to Mr. Hunter, the very reverse condition occurs, of that imagined by Mr. C. Bell; and the fluidity of the blood itself, on which depends the greater or less degree of velocity of the circulation, is not only not necessary to, but is quite incompatible with, the purposes of the animal economy. "Fluidity is only necessary for the blood's motion to "convey life; and the continuance of life is, probably, "owing to its being coagulated, and becoming a "solid. The support of the body is, at least, owing "to this cause. The moment the blood is at rest, "it begins to form itself into a solid, and changes "into this or that particular kind of substance, accord-"ing to the stimulus of the surrounding parts, which "excites the coagulum into action, and makes it form, "within itself, blood vessels, nerves, &c."*

We have, among many others, the authority of Dr. Hales for believing, that "the different secretions are "not made pleno gurgite, that is, with the full force of "the arterial blood;"† and we know, in fact, that, where the blood is much accelerated in local or general inflammations, the blood itself assumes a condition unfavourable to some of the natural secretions, which in the same proportion become diminished; while, at the same time, the growth of the body is so

* On the Blood, p. 86. † Vol. ii. p. 124.

far from increasing, that the reverse state, or that of wasting, occurs; we know, also, that, in advanced life, when partial and general diminution of activity occurs, the vessels universally become more tortuous; the retarded circulation thus probably allowing a greater facility for the slower functions of secretion and nutrition, and preventing, in vital parts, the dangerous effects of casually accelerated circulation, in vessels less capable of resisting the usual vis á tergo, and the injuries connected with a less regular impulse. We may, indeed, farther remark on this head, that where, from structure, or the purposes of growth, a larger amount and greater force of blood are necessarily determined to parts intimately connected with the life of the system, no wiser or more suitable provision can be imagined, than such a means of retardation against the injuries, which, in this, as well probably as in the former cases, must occur from excessive momentum, arising from increase of velocity, conjoined with the admitted increase of amount. The uterus, the testicle, the splenic artery and glands, the instances adduced from the organs of the human body, as having an occasional remission of their activity, or an occasional call for an increase of their function, are all, according to Mr. Bell, remarkable for the tortuous form of their arteries; but he chiefly relies, for the proof of his position, on the state of the uterus during gestation. "If," says this author, "a reason can be given, why the blood "should be retarded in the uterus during gestation, "I shall be inclined to acknowledge all the other

"instances to have no force."* Now the function of the uterus in that state is to supply the materials for an entirely new growth, and all that seems required is a sufficient supply of those materials, on which "the organic spirit" may operate. I would, however, in return, ask a reason why the circulation should be accelerated? or what connection a rapid motion has with the processes of accretion and growth? It is true, that such a process might act as a stimulus to neighbouring parts, from which the demand is made, and might increase motion; but why the vessels immediately destined to this purpose foreign to the circulation—vessels, which, according to Mr. Hunter's distinction, are not mere "carriers" of blood,—should simply suffer this change, requires a much more distinct explanation than this author has thought proper to supply. In this instance, also, we must be content to adopt the very opposite conclusions which have been advanced by Mr. Hunter, with regard to the value and uses of "serpentine" arteries.†

It appears, indeed, probable, in the second place, that, as the vessels approach their destination, where certain changes are to be effected upon their contents, they would, by some adaptation, be placed out of the course of the common circulation, and become connected with the new influences of the organs in which they are involved; so as to enable those other vital properties, besides mere motion, to be exerted in the developement of new products, whether these be the depositions required for growth and restora-

* Page 52. † Hunter pp 164, 5—175, 6—9.

tion, or the various secretions intended for the excitement or support of the different parts of the animal system. Why, however, in admitting this, Mr. Bell should also infer, that the vitality excited by this new relation should only be irritability, or should only have, as an effect, accelerated motion of the blood, it is not easy to discover; since, as has before been observed, we are not aware that any of the indicated processes can result from this power, or from this effect. Neither muscularity, nor velocity, can, as far as we know, act in producing the secretions, &c.; nor can it easily be explained, how less disturbance of the general motion of the blood should occur, because in every part, where local functions are to be performed, an artery is rendered more muscular, and more active. Surely such local activity must induce a greater general effect through the system, than would result from the retardation of the larger amount collected in the tortuous arteries, according to the view generally taken of their effects. It may be added, that, instead of putting them out of the circulating system, and, on Mr. Bell's principle, disconnecting them with it, increased muscularity and activity, and the consequent increase of velocity, seem to connect them more nearly with the supposed structure and functions of that system.

The conclusion, that tortuous arteries both retard and accelerate the blood, to which this author is reduced, from the impossibility of reconciling his own opinions with the facts of the case, does appear, notwithstanding the attempted explanation, very

"paradoxical." He concludes, that when the tortuous artery is not excited, it retards, and when it is excited, it accelerates, the flow of blood. Now, whether excited or not, a tortuous artery must, at all times, be endowed with the properties he has assigned it; the existence of which properties, under natural circumstances, constitutes, according to his view, the privilege of the seats to which the vessel is distributed. But these very properties are an excess above the amount of the same in other parts; the effect, therefore, of serpentine arteries must at all times differ in degree from that of straight arteries. But on the second head, Mr. Bell has proved to us, by Dr. Hales' experiments, that the effect of excitement is to counteract, instead of accelerating, the flow of blood.

It is needless, in this place, to examine many doubtful facts which are assumed, or farther to pursue the objections which arise at every step of our progress through the labyrinth created by this author. With him we readily admit, as we have before granted to others, that "arteries are fibrous, dilatable, con-"tractile; and that a certain proportion of this con-"tractility depends on the life, and ceases with it; "that the velocity of the blood in the different "branches of the arterial system is not the same; "that parts in disease, and under exercise of their "natural functions, have a different condition from "that which prevails under health, and ordinary "circulation." But it does not follow, that all these are dependent, as has been supposed, on simple

varieties of velocity, and slowness of the blood, or, as it is called, partial increase or diminution of action. Many of them may be different in kind from such an exhibition of function. Neither does it follow, even if this partial action of arteries cannot be denied, that their irritability and different degrees of excitement and growth constitute this difference.* These are not simple varieties of more and less, but of kind, precisely as the function of secretion and that of circulation differ from one another. It has already been observed, that no process of irritability, with which we are acquainted, will produce the growth of parts; nor of itself, as the sole manifestation of vital power, occasion this local variety and change, in seats predestined to particular local functions; though, in some cases, it may be the intermediate process between the primary power and the effect produced, in the business of secretion and growth. So impossible, however, is it to assent to the proposition, that the arteries possess the chief power in circulating the blood through the corporeal system, and that the heart is rather the regulator, than the prime and efficient cause, of the circulation; that, from a consideration of all the circumstances, it is necessary to adopt the very opposite conclusion.† The arteries seem to possess, if any, the regulating power; and the heart to be the prime and efficient cause of motion.

When, as we learn from comparative anatomy, the heart of the chick is hard and firm, and pulsating,

* P. 55. † P. 80.

while the vessels are in a completely soft and yielding state, "cum summa mollitie arteriolarum."* When the punctum saliens exists, and is discernible, at a period before the sensible developement of the vascular system; when, in the human frame,† we see that the heart alone does actually give motion to the whole mass of the blood, in cases of recovery from fainting and drowning; when, in diseased hearts, we observe the great disturbance which occurs in the circulation, and the suspension of the blood's motion arising from causes acting directly on the heart; when, in the larger number of animals, the aorta and vascular system are in direct connection with the heart, the nature of whose action is a source of immense, and hitherto uncalculated, force, aided frequently by the powerful impetus given to the blood's current, by the auxiliary power of gravitation; when we see the lungs not placed in juxtaposition with the whole heart, but only intermediately situated between the two hearts, of which one, receiving blood endowed with more vital and stimulating qualities, is destined entirely to its circulation; when we see, that, under long enduring and most extensive diseases of the vascular system, circulation continues without any material difficulty, while, as Bichât shews, such a disease, or disposition to disease, occurs universally in these textures at certain periods of life; when we attend to these and other arguments, and consider the feebleness of the proofs adduced on the opposite side

* Haller Phys. vol. i. p. 429, 30; and De Gen. Anim. Exerc. p. 17.

† Phys. vol. i. p. 428. Whytt, p. 224.

of the question, we are irresistibly led to believe, that the arteries have a comparatively inconsiderable function, as agents in the propulsive motion of their contained blood.

On the whole, there is no evidence sufficiently decisive to authorise the conclusion, that the arterial and capillary powers are identical with the irritability of other parts. If contraction, on the application of a stimulus, seems, according to some experiments, to belong to them; it is still more clear that they will, under particular states of excitement, dilate, and, according to the opinion of some enquirers, undergo certain tortuous and vermicular, or other anomalous, movements. It is very evident, as has before been more than once observed, that the arteries possess a high degree of contractility. They possess also, in a considerable degree, the power of dilatation. This power of contraction differs, however, so far from any known effect of irritability, as, under particular circumstances, to produce the permanent effect of a completely impervious condition of the tube; and that power of dilatation, in various instances of continued impulse, frequently from increased action of the heart, induces likewise a permanently enlarged caliber of the vessel, giving occasion, under certain conditions of pressure and obstruction, to the appearance of pulsation, and the phenomena supposed to be indicative of action increased.

That local contraction of a portion of a vessel may occur without increased action of the heart, is so possible, that the opposite state of dilatation is

that which we expect to arrise under such an influence. On the other hand, under extreme debility and exhaustion, the occurrence of the first mentioned effect has, not unfrequently, been observed. That local dilatation may occur without it, is also very evident, when we consider the various causes which may combine, independently of the heart, to produce local momentum.

But is there no possible modification of tonicity itself, by which local changes may be effected, though this power does not strictly coincide with that contraction on stimulus which is called irritability? Why may not the tonicity vary in degree, or be variously combined or affected by the different agents with which it is related? Such an admission has, indeed, been made, and required, by Dr. Parry.

Of some effects which may result from changes in the contained fluid, an occasion may be given to speak hereafter. On their importance, it cannot be necessary to expatiate. In that composite and vital fluid we must, probably, seek for analogies with the irritability of other parts, and a capacity of affection from many hidden causes, which, by their perpetual or occasional action, produce the admirable effects we momentarily observe in the processes of life and renovation.

The vessels, then, in every part of the system, are endowed with a vital power of contractility, which has been denominated, by Dr. Parry, Tonicity; of which the common condition is a mean state of contraction, capable of increase or diminution under the dif-

ferent circumstances in which the animal body is placed, in its relation to external or internal causes. They are also endowed with the mechanical power of elasticity, a combination which distinguishes them from most other composite parts of the body, and which is rendered necessary by the complex and varying functions they are called to perform. According to the evidence we possess, the capillaries, or smaller series of sanguiferous vessels, do not possess those powers in a higher degree than the rest; though, for certain specific functions, which distinguish them from other parts of the vascular system, they are probably endowed with powers, in regard to the circulation, somewhat different in their nature from that of the other vessels, whether, according to a recently revived opinion, that difference consist in attraction or affinity for the blood, or in some indefinable relation between the contained fluid and the containing tunics. That irritability alone is insufficient for the specific purposes to which these more minute vessels are destined, is clear, from a review of the peculiar effects of this power in all the seats where it exists. In all these seats expulsion alone seems to be the object of its agency. With the exception of that structure which composes the heart, and which seems adapted to an active dilatation, even the function of circulation could not be effected by the process under consideration. In the œsophagus, stomach, intestines, bladder, an expulsory contraction takes place without any corresponding dilatation, till this condition is assumed as a state of rest, and as a return to a state

of inaction. But to class under this head alone all the important business of the capillary system, even if we confine ourselves to the transmission of the modified blood, appears to be far too great a limitation of the powers of these vessels.

On this subject of contraction and dilatation, it appears to me that Mr. Hunter has supplied an excellent argument, why this state should not, and cannot, exist in the arterial tubes; and that it can only be necessary in cases where there is no first propelling power. He clearly shews the entire want of analogy between the action of the œsophagus, stomach, and intestines, and that of the arteries, arising from the circumstance, that the former are not endowed with elasticity. Hence it happens, that, if once contracted, they have no power of becoming elongated, or dilated again, except by the contraction of some preceding part of the canal, which propels its contents into the relaxed portion, and thus serves as an elongator. The arteries, in his opinion, have but one kind of muscular action, contraction; and their dilatation is produced by elasticity alone, of which the effect is to preserve a determined or middle state. In the above examples, it appears to him, that a first propelling power, such as a heart, would have been unnecessary; for as there must be a succession of expulsory contractions and dilatations, its influence would have been immediately lost. Now it may be asked, in what the supposed succession of vascular contractions and dilatations differs from this state, and from these conditions? and again, what purpose this determined

and middle state of Mr. Hunter is to serve, if not for the circulation? or in what respect it differs from that of Dr. Parry? Here, again, it is evident, that we must possess some more precise knowledge, of the supposed successions, than the vague account, with which writers have hitherto suppplied us, before we can be prepared to argue this point at greater length.*

That, being endowed with a variable contractility, with tonic and elastic powers, such as have been amply described in the Experimental Enquiry, these vessels, with the rest of the vascular system, should be destined to perform some important office in the circulation, is sufficiently manifest. But, it will not be readily admitted, that this function is analogous to that of the heart. In the first place, examine the state of the vessels said to be induced by the application of stimuli; and, to say nothing of the great uncertainty and variableness of the effects produced, take the most favourable result, a contraction, in itself highly variable as to amount and duration, and then declare, whether this be calculated to carry on the circulation in accordance with the uniform and measured action of the primary organ? Whatever fitness there may be in such a contraction, for emptying the vessels of their contents, there is none for the admission of fresh supplies, or for the continuation of any regular circulating process. Look again, at that other undefined and indefinable change in the vessels, called contraction and dilatation! Unless it can be shewn, that, under its exhibition, a near

* Mr. Hunter's Treat. on the Blood, &c. p. 108.

approach is made to similarity of form, a near coincidence in action with, and resemblance in structure to, that of the heart, it is evident, that, as Dr. Parry shews, such change must impede, rather than promote, the circulation of the blood. Under the real state of the vessels, will any one be bold enough to maintain, that, because the heart dilates and contracts without interrupting the circulation, the arterial tubes can, therefore, do the same? What similarity of structure, it may, again, be asked, warrants the assumption of such identity of function? Are there clear and distinct recipient cavities in the one, as in the other, guarded by valves, which are called into operation during the alternating movements of those double chambers? Is there, in the arterial seats of dilatation and contraction, what may be called a compensating irregularity, so that, as happens with the auricle, under circumstances of disturbance, the one part may possess the faculty of distinct and more frequent motion than the other? The want of sameness of effect, when these vessels are removed from the body, has already been noticed. The attempt to establish an analogy between the parts in their natural situations is just as vague and unsatisfactory. This absence of analogy may, indeed, be carried through all the facts of the two systems. The heart, as has been shewn by Haller, is capable of motion in all its parts, "totum cor mobile," and is most admirably guarded against pressure and irregular external influence, by its strong protecting shield. The arteries have no similar power of motion, but, wrapped in

a densely adhering and connecting cellular membrane, are momentarily subject to contraction of diameter from pressure, extension from elongation of their axis, and all the influence of an ever varying gravitation ; a fact, which, in itself alone, establishes a complete distinction between the heart and the arterial tubes.

The impediment supposed to arise from alternate contraction and dilatation of the arteries is founded, it has been asserted, on the supposition, " that the " ventricle, when it acts, has to overcome the con- " traction of the arteries, which is not at all warranted " by facts ; for there is much reason to believe that " the middle coat of the arteries is so constituted, " that its fibres contract and relax in such a manner " as to assist the heart, and promote the circulation." This description is not very explanatory or intelligible. When the ventricle contracts, the artery in its immediate neighbourhood must be under one of the assumed conditions. It must be either in a state of contraction or dilatation. If of the former, it can scarcely be conceived that such a state, with whatever rapidity it may be changed for the purpose of facilitating the entrance of the blood, does not rather oppose an obstacle, than afford an assistance, to the circulation. If of the latter, a certain amount of blood is still to be displaced, which is found in this comparatively inert portion of the vessel; beyond which, again, is met all the resistance, which must proceed from a contracted portion of the same tube. We have certainly no evidence, that by any auto-

matic and self-regulated movement, the balance can be uniformly preserved between these organs of unequal force, and of very disproportionate action. Till the difficult problem, the nature of the arterial contraction and dilatation, be completely solved, it cannot, however, be expected that we should define wherein the difficulty consists, or wherein the difference lies between our view of the case, and that which has been taken by the authors already quoted.

The vessels endowed with these powers have, as before observed, to perform a very important office in the circulation. As the "carriers" of the whole circulating mass of blood, they are subservient to its uses, and to the varying conditions in which it is placed from changes of a chemical or mechanical kind, according to the vital demands of the system, or the accidents to which it is liable. From their structure, these tubes are admirably suited to undergo a constant state of accommodation to the column of blood, impelled by the vis à tergo, or modified by local accidents. By this faculty of accommodation, resistance and recovery, according to the varying pressure of the surrounding parts, and the changing force of gravity, they preserve a well regulated momentum throughout the system ; and by the power of longitudinal extension, adapt themselves, under the forced positions, in which they are frequently placed. By the nature of their internal tunic, which probably greatly diminishes the resistance of friction, they suffer the blood to pass without much interruption, and greatly facilitate the progress of the current, to

which the action of the heart, assisted mainly by its strong expulsory shock, and often by the gravitating descent of the blood through the wide aortic canal into the inferior parts of the system, has given so powerful an impulse.

What other means occasion or promote the circulation of the blood, it is not necessary to enquire in this place, nor by what mechanical contrivance, in this beautiful apparatus, the uniformity of the current is supported. That an alternate contraction and dilatation could not produce this effect, is sufficiently evident. We are, perhaps, not yet accurately instructed in the real action of the heart, which, according to Haller's repeated observations, has not always a regulated alternating action, and we are as little acquainted with the supposed contraction and dilatation of the arteries. But neither separately, nor in conjunction, could such conditions admit this exact and regulated effect. A very able author in the Medico-Chirurgical Journal, has taken pains to shew, that an intermittent or interrupted action does, and under all circumstances must, occur in the larger vessels, or else that a vacuum must be allowed to exist in some part of the series. To the latter we have no objection; but we have all the evidence of sight, that in vessels much beyond those of a capillary size as well in cold, as in warm-blooded animals, the motion of the blood is perfectly uniform; and that this uniformity is only destroyed, when the action of the heart is lessened, or the current is impeded. It may be added, that though, from the

use of the microscope, some deception may arise as to the general rate of velocity, there can be none as to any comparative state between two parts viewed in the same manner, and at the same moment. An interrupted or intermittent circulation must be as evident under the microscope, as without it.

It has been observed, that if the current of blood is rendered slower by ligature on a limb, or otherwise, the circulation proceeds by jets; which seems to render it probable, that the whole regularity depends on the heart. But it has, on the other hand, been noticed by some authors, that, even after the removal of the heart, the circulation proceeds with perfect regularity, for a considerable length of time. There is much difficulty in explaining this fact; for as the contraction of the arterial tubes, whether resulting from tonicity or irritability, will not admit such a continuance of circulation, so neither, à fortiori, will the supposed alternating movements of these vessels in any way account for the entire absence of all corresponding interruption in the force of the circulating fluid.

That on the healthy state of the sanguiferous tubes the due performance of many important functions must depend, both in health and disease, is as evident as their necessity for the transmission of blood. The morbid affections, to which, with the rest of the body, they are subject, and the modes of vitality common to them with all other vital parts, must specifically influence the function in which they

are perpetually engaged. That some of the states of disease, in which they are implicated, do not entirely belong to them, or that the cause of some well-known morbid effects does not, as has been imagined, consist exclusively in affection of their vitality, may be rendered probable, when the subject of Inflammation is considered.

CHAPTER II.

INFLAMMATION.

On the nature of the powers inherent in, or connected with, the vascular system, will naturally depend the effects and relations of the same system in the processes of disease. It has never been denied, that there is, throughout this system, a capacity of contraction. Under all doctrines, the result is, in fact, the same. Notwithstanding frequent inconsistencies, which seem interwoven with that theory to which we are chiefly opposed, contraction is, probably, even with the advocates for that doctrine, the real effect of the causes exciting irritability; and it is no less derived from the exertion of that property, which, from its want of analogy with other known powers, Dr. Parry has, in distinction, called Tonicity. The power of dilatation has also been considered of no questionable existence; and in the Experimental Enquiry its nature has been accurately defined, as a power constantly co-operating with the vital contractility in preserving an effective state of the system,

under the numerous contingencies to which this latter is momentarily exposed. It is, however, very clear that in denying the existence of irritability, strictly so called, we must, also, at once discard the supposed effects of stimuli in destroying, under a perfect state of the organization, this supposed vital power of the vessels, and in this manner giving occasion to the congestion, constituting, on this theory, the essential condition of inflammation. Much less can we ascribe solely to the same cause, or to an assumed debility of the vessels, the changes which, to every observer, are manifest in the blood itself, when placed under the well-known circumstances of experiment.

The first effect generally visible, under the causes inducing the state called Inflammation, is an increased circulation of the blood. To use the words of the author before quoted, "temporary excitement of the "capillaries generally precedes" the other effects. "Those agents which produce inflammation, first "excite the action of the vessels." A repetition of the same application produces a change in the blood itself, of a very marked character, coincident and contemporaneous with which is, to all appearance, a dilatation of the vessels, and perhaps, in some part of the series, a slower motion of the blood. The principal question for consideration is the cause of this dilatation of the vessels, a circumstance which we are disposed to admit, without taking advantage of certain alternatives, which have not unfrequently thrown a suspicion on the accuracy of the very fact itself. In repeated observations it has, indeed,

been a matter of doubt, whether, in cold-blooded animals, such apparent enlargement were not a deception arising from the circumstance, that, from the changed colour of the blood, the whole caliber was now become visible, which before, from similarity to the serous and colourless fluid, had not been distinctly defined; and the greater fulness of the inflamed part has, on other occasions, appeared to arise from vessels now containing blood, which previously had been wholly, or in part, empty. Admitting, however, this enlargement and dilatation of the capillary or smaller series of vessels, it is evident that we cannot ascribe it to the sudden loss of a power, the existence of which we deny.

We are, indeed, much more inclined to attribute the increased capacity of the vessels in these cases to an increased volume and altered constitution of the contained fluid, than the increased volume and visible changes in the blood to any modification of the vital power, similar to that which is expressed by the term Debility. While we necessarily exclude this condition under the view which its advocates have adopted, it may, also, admit a question, whether, under our own, this particular term could safely be employed? and whether, though from increased volume, by whatever causes produced, the vessel is less able to perform its functions, and therefore may sustain some relative loss of power, so positive a term would not be incorrectly applied? We might indeed, by the same process of reasoning, ascribe debility to a strong man, whose arms were held by one stronger

than himself, as apply such a condition to the vessels, whose inability has a reference only to the excess, or altered quality, of the contained blood. Under this view, the unqualified term Debility conveys an inaccurate and deceptive notion; as, the moment the blood resolves itself, if such a fact ever occur, the original power is resumed, unless by the possible continuance of interrupted action, and forced distention, such natural power be destroyed, or some change take place in the organization of the part itself. This opinion is directly the reverse of that maintained by Dr. W. Philip. It may, perhaps, be found more correct, more consonant with facts and appearances, and more explanatory of the phenomena, than that which may strictly be called the doctrine of Debility.

It is said, that inflammation does not occur under the state of increased circulation, but that it occurs as an effect of the continuance of the causes which produced increased velocity. "Certain stimuli "produce an increased velocity of the blood's motion, "and a contraction of the blood vessels. If the "stimulus be long continued, or increased in power, "the small vessels, which, in the natural state, admit "only one series of globules, become so dilated as to "*allow* an accumulation of a *much less fluid* and "*redder* blood in them, which *loses* its *globular* "appearance, and moves much more slowly than that "which previously passed through the vessels." This may be a sufficiently fair view of the production of inflammation; but it does not follow, that this effect

of dilatation, from the repeated application of the stimulus, is the consequence of exhaustion. In numerous instances, advanced by our opponents themselves, the effect of increased circulation is dilatation in the succeeding and connected series of vessels, which mechanical effect, permitting a congestion, would also induce a comparative retardation of blood, and by a continuance of the same causes, all the effects consequent upon an altered condition of this fluid, which itself then becomes an agent in the production and maintenance of the other inflammatory conditions. It is true, that under the usual circumstances of continuance, increased rapidity of the circulation alone is not productive of inflammation; and it is an equally familiar fact, that local inflammation may be induced, without any general increase of circulation, by causes acting on the relations of particular seats. Dr. Parry has shewn, that "permanent increased dilatation, following excessive "momentum, does not generally occur, till sometime "after that momentum has increased." It is, however, also sufficiently evident, as to the first point, that, if continued beyond a certain degree, this state, or its alternatives, sanguineous or serous effusion, will follow, and that it is generally owing to the short duration of the state, that such effects are not more frequently produced. Under a continuance of increased rapidity of circulation, inflammation would, on many occasions arise in consequence of local dilatations, whether mechanically or otherwise produced, according to predispositions (an ample and intricate subject) or

tendencies recently assumed from the situation, structure, or functions of particular seats; which local changes might themselves induce an inflammatory condition of the blood. In this case, dilatation precedes, indeed, the change in the blood, and is the apparent cause by which it is effected. But it is clear, that the vitality of the vessel is not concerned as a cause, though to a modification of the contractile power is to be ascribed the change which takes place, and which is one of the accompanying conditions of inflammation. Increased circulation may, unquestionably, produce dilatation of the vessels, and consequent retardation of the blood. But another alternative of the effects of hurried circulation, as productive of inflammatory conditions, would be the immediate effects on the blood itself. That the more rapid evolution of heat, and the effects of increased momentum, should not, in some respect, modify the mass so agitated, is, indeed, à priori, highly improbable; and that a change may be so induced from the continuance of a rapidly increased circulation, even without any apparent local affection, is evident from the state of blood in habitual drunkards, in pregnant women, and in various diseases usually called nervous, which exhibits, when drawn, the well-known phenomena indicative of this state.

But, with regard to the second point, the effect of dilatation is said also to be suddenly produced, in some instances, apparently without the intervening process of increased circulation. If the stimulus be of a very acrid nature, " the blood in all the smaller

"vessels becomes *very red*, circulates very slowly, "and in some vessels stagnates without any previous "excitement." If such be the case, the dilatation still appears only as a concurring effect with the change of the blood into a homogeneous red mass, in which the colouring matter unites, the globules disappear, and the character of the fluid becomes materially altered. It is not impossible, that this change may, in such cases, be the cause of the dilatation; and, while it is the only process which can be admitted, under the theory now adopted, it is, at least, as probable as the opposite doctrine. Admitting the principle of irritability, and the supposed explanation of the effect, the debility may, in this instance, as logically and consistently be called the consequence as the cause of the phenomena. The loss of tonic power, or of irritability itself, is likewise as commonly the effect of distention, as of any other causes; and, under natural circumstances, without disorganization, is perhaps usually so induced in the heart, stomach, intestines, and bladder. But Dr. W. Philip himself, gives up, in a great measure, the doctrine of debility as the consequence of over excitement by stimuli.* "*Any cause* lessening the "action of the capillary vessels causes inflammation." Among such causes, then, he admits the mechanical effects of distension, of obstruction, and interruption, as well as the influence on the vital principle from diminished irritability. He has also directly asserted, that "increased flow of secreted fluid, we have

* Quarterly Journal of Science and Art, xiii. p. 107.

"reason to believe, from the modus operandi of certain "causes of inflammation, would often occasion morbid "distention."* If it be, then, admitted, that these powers are diminished, it by no means follows, on this account, that they are primarily affected. It would, perhaps, be unjust, in the present, as in the former, case, to characterize the state induced as one of debility, on its first accession; though, under a continuance of the same circumstances, such debility, whether of tonic or irritable properties, may, ultimately, supervene.

But setting aside the presumed influence on the irritability, it has been admitted by Dr. Parry, that causes acting on the tonicity, of the vessels, may be productive of local changes, and may, therefore, be intrumental in effecting the condition called Inflammation. And, who shall deny to any one vital faculty more than to another the possibility of modification, though the nature of such change cannot be exactly defined? What explanation can the advocates for irritability give of this original or modified faculty, to warrant, on their part, any enquiry as to the nature and modes of tonicity? Without assuming a contraction of the arterial tunics, on the application of a stimulus, as their specific property, it is surely not irregular to assert the want of tonic power, as a condition to which those parts may be liable. The muscles which compose the cheek, under certain circumstances, lose the faculty of mean tonic extension, and the tunica dartos supplies examples of many of

* P. 273.

those motions, and conditions of relaxation, which, however they may resemble its effects, do not proceed from irritability.

Having before examined some of the facts connected with this subject, which we have been inclined to think less correctly stated, or less conclusive, than has been imagined, and being also disposed to question the accuracy of the distinction made by Dr. Wilson Philip, and the advocates of his theory, as to the supposed relation of cause and effect in the processes of inflammation, let us particularly enquire what are admitted as symptoms or evidences of the state so called, and how such symptoms can be explained, according to the view which has been taken of its proximate cause. The proximate cause of inflammation is considered, by this theory, to be a debility of the capillaries, followed by "increased "action," or, to use Dr. W. Philip's definition, "a more vigorous contraction" of the larger arteries. In consequence of this condition, the vessels of an inflamed part are greatly dilated, and the motion of the blood becomes extremely languid, and where the inflammation is greatest, ceases altogether. Now the symptoms which are generally admitted to afford evidence of an inflamed state are redness, swelling, pain, throbbing, and increased temperature. The question of increased pulsation, or throbbing, in a state of inflammation, is one which evidently perplexes the theory of weakened action; for how can this be consistent with every degree of slowness to complete stagnation, which is declared to be the state

of the highest degree of inflammation? To account for the presence of this symptom, the theory is changed, and the essence of inflammation does not consist in weakness of the capillaries; in other words, the capillaries are not the seat of the symptoms, but an increased action, a more vigorous contraction, of the larger arteries, must be taken into the account. If, indeed, we give credit to Dr. Hastings, the larger arteries are not always implicated; an admission which appears as little reconcileable with consistency, as if we were to say, that the symptoms of inflammation do not always accompany inflammation. In this way, however, two sorts of inflammation are adopted, essentially distinct from, and opposed to, one another, and neither of them explaining the local effects; for, a more vigorous contraction which is not coincident with, but follows, a complete cessation of motion in the capillary system, and which, according to Dr. Philip, necessarily excludes the idea of increased quantity and accumulation, can no more furnish such explanation, than the absence of motion in the inflamed part, without such interference of the larger arteries, is capable of so doing.

In the midst of these difficulties, Dr. Hastings takes still another view of this symptom, and attributes the throbbing to a "large volume of blood propelled "into the dilated capillaries." Here he appears to be at direct variance with himself, and with Dr. Wilson Philip; for if the capillaries were primarily so much dilated, in consequence of debility, as entirely to disable them, and induce a complete stagna-

tion and obstruction of their contents, how can a large volume of fresh blood be admitted? or, if it could be so admitted, and it were true, that the debility of the vessel produces the change on the blood which has been ascribed to it, how could the symptom of increased pulsation continue, after the first impulsion of fresh blood which fills the weakened vessel?

Again, if the vessels are not so obstructed by stagnating blood, and admit this propulsion of a large volume, according to Dr. Hastings' opinion, in what respect does this effect differ from the increased momentum of Dr. Parry? which, according to a certain rate, may exist either under diminished or increased quantity, or under diminished or increased velocity; and on the vascular tunics might, in either case, induce an effect corresponding with the symptom in question. If the blood in the capillaries be not in this state of entire stagnation, which Dr. W. Philip considers the highest degree, and, therefore, the very essence of inflammation; and if, as Dr. Hastings maintains, the vessels do still admit a large volume of blood, so long as this important symptom lasts; we may, indeed, probably ascribe it, with Dr. Parry, to increased momentum of blood, though we cannot immediately connect it with increased action, a state which may be entirely opposed to the former condition. Admitting, however, an entire obstruction in the capillary system, from whatever cause this may arise, increased momentum will occur in the part immediately preceding this seat of disease,

and produce dilatation from accumulation, and a corresponding or consequent tonic exertion of the vascular tunics, thus giving rise to the symptom of throbbing, a pseudo-alternating state of contraction and dilatation. The symptom, then, is neither increased action, nor diminished action or debility, but the increased momentum of Dr. Parry; and without this condition, it does not appear that the state of active inflammation can exist.

In considering the swelling and enlargement in an inflamed part, it is certainly true, as was observed by Haller in answer to Whytt, and as has since been repeated by Dr. W. Philip, that, under an increased action, or more vigorous contraction, of the vessels, a larger quantity of blood cannot be received into them. Under such a supposition, this symptom cannot then be explained; the capillaries alone are, therefore, generally charged with this phenomenon. Dr. Hastings here again takes an opposite view, and as he attributes the increased pulsation of the carotid, under inflammation of the tonsil, to accumulation and increased action, so does he, also, ascribe the symptom of increased dilatation to a large volume of blood propelled into the already dilated capillaries, by the left ventricle of the heart. The dilated state of the capillaries, if it exist, might certainly be sufficient to explain this phenomenon, whether that dilatation were the consequence of debility, or of mechanical force. If, in any part of the series of larger vessels, accumulation do also occur, as has been supposed under the last head, it

will still further assist in ascertaining the cause of the symptom. It may not be irrelevant briefly to notice in this place, that, whatever may be the nature or effect of increased action, a stimulus does not always necessarily either contract the arteries, or act by inducing debility. Its early and immediate effect is to produce local fulness, in numberless cases where there is not the slightest reason to suspect obstruction, or any particular influence on the capillaries. If we empty the vessels at the back of the hand by holding it up, and thus occasion their usual contraction on the reduced amount of blood, we can immediately cause them to become turgid with blood, even against gravity, by bringing the hand, in the same posture, to the fire. Will it be said, this is the stage of excitement? It is an effect, then, wholly opposed to the principles advanced with regard to the nature of this condition, and its connection with a contracted state. Will it be said, that it is the stage of debility? Here is no evidence of stagnation of the blood, or of an approach to stagnation. In this instance, we have dilatation or swelling, with excitement, and without debility, and in it we have no proof that any circulation, but that of the larger vessels, is implicated. Will it be said, that this is not inflammation? The same condition may, however, be continued, till a permanent state of inflammation is induced; and the swelling will not be the less palpable in the larger vessels, whatever may take place in the capillary series. But if this be not inflammation, it is clear, that the symptom of swelling, as a constituent of this state, need not pro-

ceed from debility, its assigned cause. To vital or mechanical distension of the vessels, to changes induced in the blood itself in the early stages, and to many other causes, as the disease advances, we may safely ascribe the symptom of palpable local enlargement, without any reference to the assumed state of debility in the vessels, as the sole and paramount source from whence the subsequent phenomena are derived.

The redness of an inflamed part has, on the same grounds as those adduced with regard to the former symptoms, been referred to an increased action of the vessels, which is supposed to diminish the amount of the fluid contents. This symptom is, therefore, again wholly thrown on the capillaries. The redness which appears on the early application of a stimulus, is, certainly, coincident with a dilated, not with a contracted, state of the vessels ; while a pale colour is, on the contrary, an evidence of contraction. That, in this case, the symptom is not the effect of debility, is probable, not only from its early appearance, but because the very appearance itself, under circumstances of common excitement, is admitted as an indication of health and vigour. Here, then, as in the last instance, is redness with dilatation, and without debility ; and if local swelling and throbbing are compatible with such a dilatation, we may also admit this symptom to exist independently of the same cause. It is probable, that besides mere amount, such heightened local colour arises from the changes in the blood itself, which are, at least, as evident as the vascular dilatation ; and it may also be

connected with an error loci, or, if not strictly with this circumstance, with an unusual aggregation of red globules in certain minute vessels. Allusion has already been made to the well-known occurrence of such changes. The latter circumstances, if they exist, may be connected with augmented impulse, mechanical distension, or modified local tonicity ; and none of these causes will have any connection with debility, as a source from whence the effects follow.

On the subject of increased temperature, it is not easy to omit the conclusions of Mr. Hunter,* who says, that from all the observations and experiments he had made, he did not find that a local inflammation can increase the local heat above the natural heat of the animal, and that he had never seen a case, where the heat was so much increased as it appeared to be to the sensations. Admitting, however, such local increase, the explanation which Dr. W. Philip has given, does not seem altogether satisfactory. According to his doctrines, two parts are engaged in the phenomena of active inflammation, —the larger vessels in which the action is increased, or a more vigorous contraction takes place ; the lesser ones, in which stagnation occurs. Why, á priori, should all the charge of increased temperature be thrown on the debilitated capillaries? It is not easy to reconcile the idea of augmented heat with such a state of weakness. Experience inclines us rather to believe, that coldness is the usual effect of a languid or stagnating circulation. " The waste of the principle

* On the Blood, p. 293.

"by which caloric is evolved from the blood is, how-
"ever, according to this ingenious author, about
"three times less in an inflamed part, than in the
"same part when sound; so that we readily account
"for the increased temperature, and arterial colour
"of the blood, although the rapidity of the circu-
"lation is greatly lessened." On this principle, then, it is inferred, that the amount of sensible heat, in a part where the blood stagnates, is as much more than in a state of natural circulation, as the degree of suspension of motion exceeds the degree in which blood is transmitted in health; so that, under entire stagnation, the highest grade of augmented temperature would be found. If this principle hold as to one degree, it must, of course, be applicable to all; and if under inflammation, it be admitted, that there are degrees of motion, the same principle should extend equally to all cases of obstructed circulation, since both equally exclude any effect of increased temperature arising from the changed state of the contained blood itself. But to omit the paradoxical proposition, that increased heat is necessarily connected with a stagnating circulation, how can we explain the circumstance, that, in a part cut off as it were from the circulation, not only the natural temperature should be preserved, but that a larger amount of fresh heat should be actually generated, than in the surrounding parts? for, it is very evident that in the state, which is considered by this doctrine the highest degree of inflammation, there can be no influx of fresh blood, or of the caloric

which is generally believed to be conveyed by the act of circulation. Whatever may be the real source of heat, or whatever its real relation with the blood in the vessels, if we look at the effects of excitement, —the increased heat, for instance, in the cheeks of a robust child after exercise,—we shall not feel inclined to say, that an increased local temperature, even such as we find it in inflammation, is the necessary effect of vascular debility. The doctrine of increased momentum, by admitting varieties of quantity and velocity, and the communication of heat in a certain ratio with the circulation, however little explanatory of all the circumstances, appears more consonant to probability and analogy, and more in accordance with usual occurrences, than this doctrine of torpor and inaction. It is, at all events, evident, that, under the influence of an increased momentum, more heat may circulate through a part in a given time, than under natural circumstances, whether we admit an increased velocity as the source of this increase, or the equally possible fact of an increased amount. On this subject we may add, that Dr. W. Philip appears somewhat at variance with himself. He has elsewhere shewn that "temperature depends immediately on the state of the circulation, and is "proportioned to its vigour,"* and that it "rises in "proportion to the increased action of the heart and "vessels;"† thus depending no less on the power of circulation, than on the nervous system. An objection to all these doctrines must, it is evident, be

* Quarterly Journal, xiii. p. 96. † P. 274.

found in the circumstance, that they wholly exclude the altered state of the blood as a probable cause of some of these important phenomena.

It appears as if all the symptoms indicating the existence of inflammation may occur under a condition the very reverse of that assumed as its cause. If, however, these states and conditions be not inflammation itself, how shall we distinguish this disease? Not by the assumption of an unproved obstruction, dependent on an assumed debility, which, as a cause, is not explanatory of the symptoms, and, as an effect, is dependent on a modified property, which itself cannot safely be ascribed to the vascular tunics. It is not difficult to shew, that, according to the case exhibited, no proof is, in fact, established of a primary or necessary obstruction of the capillary system, or of the existence of its supposed cause.

On this subject, we may refer to a still more important qualification of the doctrine of inflammation, which is admitted by these authors themselves. It is, by them, shewn that inflammation is not a property of the capillaries alone. Arteries and veins are no less affected by inflammation than these minute vessels. Upon what ground should opposite effects be produced in these two systems?—if, indeed, it be philosophical to consider them as distinct from one another. Why should that be inflammation in one, which is not so in another part of the series? and this, notwithstanding an entire similarity of appearances and symptoms in one and in the other series. As, then, all the symptoms can occur without this

supposed and questionable cause, we may, with much probability, and in proportion to the proofs against the existence of such cause, logically decide that there is some other origin of the phenomena; and, till it be more completely refuted than has yet happened, may be disposed to adopt the relative or positive increase of momentum of Dr. Parry, as an important and even necessary link in the series constituting the inflammatory conditions. That more remote effects of a primary affection may occur, unaccompanied by many of the symptoms already enumerated as indicative of such conditions, is very evident. As far as we can judge, these effects do not, indeed, embrace the question at issue. It has already been hinted, that the writers, before noticed, admit two different kinds of inflammation. In reference to this immediate part of the subject, it may be necessary to examine more particularly the recorded opinions of Dr. Parry himself, for which, after these preliminary observations, we are sufficiently prepared.

According to Dr. Parry, the symptoms of inflammation are durable preternatural fulness, and consequent distension, of the vessels, increased redness, heat, and tenderness. He endeavours to prove, from the temperament and age usually affected, from causes producing increased impetus, from local inflammation actually produced by such causes, from the greater quantity of blood, and a velocity as great in parts which are inflamed, and from the jets of blood from the cephalic vein under gouty inflammation of the wrist, that, in *certain stages* of inflammation, an excessive

momentum of blood exists in the vessels of inflamed parts, which must be considered an indispensable cause of what we see in inflammation. He considers, that the immediate cause of local predisposition is a proneness to dilatation in the vessels of the part liable to such maladies. He is so far from asserting that the morbid dilatation is the mere mechanical effect of general increased impetus, that, on the contrary, he endeavours to shew, in various places, that the fulness, constituting part of the local momentum, often accompanies a dilatation of vessels, arising from causes acting merely on their own tonicity, without any increase of the vis à tergo from the heart; and even that the increased action of the heart often follows, instead of preceding, the excessive local dilatation: thus, that, though inflammation may arise from general increased impetus, it may *sometimes occur without it.* He considers the capillary system, in all the different textures, to be the one primarily affected in this morbid change. Dr. Parry farther shews, that different changes occur in different periods of inflammatory disease, so that the phenomena may at one time be essentially different from those previously existing; thus, in a phlegmon, tonicity of the capillaries might be lost, after undue dilatation. In speaking of excessive momentum in inflammation, he refers to what may properly be called the acute stage of the malady, which is essential to the production of all the subsequent phenomena. This view of Dr. Parry's doctrines shews how very erroneous an exposition of them has been

placed before the public, by some of the writers who have attempted a review of his doctrines. Will it be imagined, that the following remarks have reference to these doctrines? "With regard to the explana-"tion of the phenomena of inflammation lately given "by Dr. Parry, which supposed an increased momen-"tum of blood, it may be remarked, that that "explanation is founded on the opinion which that "physician maintains in the 198th paragraph of his "Elements of Pathology and Therapeutics, that the "blood is not moved in the capillaries by any con-"tractile power proper to them, but by the impetus "which is given to it from the heart. This view of "the circulation in the smaller vessels has been "before shewn to be incorrect, and consequently no "time need be here devoted to the refutation of that "very ingenious, though rather mechanical, view of "inflammation." This is certainly a very summary mode of disposing of an important question, and one which is not unfrequently adopted in the speculations of certain authors. Dr. Parry's doctrine of increased momentum is, however, said, in the first place, to be founded on the opinion, that the force of the heart alone is sufficient to circulate the blood. Whatever truth there may be in the assertion, that such is his belief with regard to the circulation, what has such a belief to do with his doctrine of inflammation? How does it agree with his opinion, so frequently repeated, that the local circumstances of inflammation have as often nothing to do with the force of the heart as with it, and with his conviction that

such circumstances are dependent on a local modification of the vascular contractile powers? Without such an explanation, and such admissions, can it not be conceived, that, the power of the heart remaining the same, local circulation may be altered even by mechanical causes increasing the amount or velocity of the blood, much more by the vital tonicity of the parts locally concerned in the different processes of life? But, has it ever been shewn, that such process of local circulation is carried on by the capillaries, to the exclusion of an equal, or greater, affection of the other series of vessels. Certainly not by any of the experiments published in regard to this point. Increased velocity has appeared in them as in other vessels; but certainly nothing has been discovered in favour of a particular, exclusive, or even of an equal share of power, with that degree which is manifest in the arteries, or in the veins. This assumption must, therefore, as before said, be still considered without proof, and rather a result of the supposed necessity of the case, as it was adopted by Whytt, Senac, and others, than of any clear and indisputable evidence.

Dr. Parry's view of inflammation is, in the next place, called a mechanical one. What! the vital tonic agency which occasions dilatation, or contraction? the exertion of that vital power, without which, under either alternative, inflammation could not occur? for, though this state may arise without increased impetus from the heart, it cannot occur as a consequence of any remote cause, without a modification of that vital contractility in the vessels of the

part, on which itself depends the predisposition or proneness to dilatation.

But let us examine the evidence of the experiments themselves, which have been adduced to disprove the positions of Dr. Parry; and see whether, in the acute stage, or what is called active inflammation, the only case to which this author alludes, there is any thing to warrant a decision so hasty, and so illegitimate; and, whether the doctrine of increased momentum, as constituting one link, in the important chain of circumstances, necessary to the production of this effect, be so entirely absurd and unfounded, as is implied in the passage above cited. It has already been noticed, that, upon the evidence of the opposite writers, those agents which produce inflammation first excite the action of the vessels. Temporary excitement of the capillaries, it is said, generally precedes that debility of those vessels, which is inseparable from inflammation. In one experiment,* the Liquor Ammoniæ produced contraction of the artery, and quickened the motion of the blood, whilst the capillaries beyond were dilated, and the blood in them moved slowly. Even when the artery returned to its natural size, they continued dilated, though no application had been made to these vessels. It will, indeed, appear from various other experiments, that we cannot excite the capillaries alone, and that every manifest effect is, perhaps, only that of a quantity of blood thrown into the capillary circulation by excited arterial action, greater than

* Hastings, l. c. p. 52.

can be immediately transmitted; and that hence may occur accumulation, dilatation, and certain changes produced either by such stagnation, or by the causes themselves first inducing the excitement of the vascular constitutions.

In another experiment,* dilatation in the vessels of the web of a frog's foot, followed a quarter of an hour's increased velocity, produced by scratching and puncturing the parts with a needle. Surely this and other experiments prove more than was intended; for, in the first place, this effect in the capillaries is produced without any simple affection of the vital properties, such as is presumed to be the effect of causes inducing debility; and, in the second, even disorganization itself, a tolerably weakening process, did not produce the desired result, till the velocity had been increased for a quarter of an hour. The first result is exactly that, which is presumed to occur in the generality of instances. Can it be wondered, that, in the second case, the blood should be changed, and stagnant, in parts to which such violence had been applied, or that dilatation should occur in such vessels, under such affections?

In an experiment,† by the same author, after four immersions of the web in water at 110°, occupying two minutes and a half, during which the velocity of the blood was increased, the vessels dilated, the blood moved slower, and became redder in the arteries, veins, and capillaries. Surely this effect, on the three orders of vessels, might have been predicted,

* P. 79. † P. 81.

and the event explained without having recourse to the doctrine of debility; whether we are disposed to assume a change in the blood itself, or the usual effects of increased rapidity of circulation in some part of the series of vessels, or adopt, with Dr. Parry, a modified tonicity as the cause of the phenomena.

We learn that, in another instance, arteries, veins, and capillaries, were all dilated, and the blood became red in them, after five applications of ice for twenty-five minutes, during which the motion of the blood was quickened in most of the vessels. In the veins, however, which receive blood from the contracted arteries, the blood moved slower. In this example, a change was effected, as usual, in the blood, and the vessels were dilated. It shews also, like others of the same author, that inflammation is not a property of the capillary system alone, but, after increased circulation, alike implicates the other orders of vessels. "Sometimes," says this author, "the "large arterial and venous trunks become *the seat of* "*inflammation*;* they are dilated; the blood moves "slowly, and is very red; these vessels are much "inflamed."† At other times, the small arteries and veins are so occupied by inflammation. Yet it is also said, with some degree of inconsistency, "a "sensible enlargement of the more evident arterial‡ "and venous branches, attended with a bright red "colour, without any dilatation or redness of the "capillary vessels, is not a state of inflammation."

* P. 84—5. † P. 93. ‡ P. 102.

Inflammation, surely, is always such. If the larger arterial and venous trunks can, as is asserted, become the seat of inflammation, this condition in them must be the same, whether the capillaries be, or be not, affected.

The Liquor Ammoniæ was applied, in another case, for five minutes, and afterwards for ten minutes; the velocity was increased, and then the vessels dilated, while, as might be expected, the untouched vessels remained in their natural state. As far as cold-blooded animals are concerned, to which his remarks do not relate, Dr. Parry could himself have wished for no stronger confirmation of his doctrines than these experiments; which prove, as he does, that dilatation is a consequence of the causes producing inflammation, and that, in many cases, these causes first act by inducing increased momentum of blood. In these very marked instances, increased velocity universally preceded the dilatation, and we have not any attempt to shew that the supposed contraction so far diminishes the circulating amount, as to exclude the ratio within which an increase of momentum may occur. It must, also, not be forgotten, that, though we admit the effect of velocity increased, we do not connect it with contraction of the arteries, as a consequence of the application of stimuli; and, moreover, that, in cold-blooded animals, we have never witnessed that contraction. The cause of that increased velocity we may be disposed, still, to place among the mysteries of the animal system, and, at all events, to attach a much greater importance to that highly vital fluid, the blood,

than to the vessel which is the mere organ of its conveyance. Those who advocate the opposite doctrine, imply and admit, in fact, no vital action in the blood, which is not connected with, and dependent on, its containing vehicle.

In the cases hitherto cited, the dilatation is too evidently unconnected with the predisposition of the parts, and too equivocally allied to vascular affection, to allow any proof to be drawn in favour of diminished irritability, as a cause of the subsequent phenomena of inflammation.

In the following experiments no mention is made of increased velocity as a previous symptom. In three minutes after application of the Liquor Ammoniæ, the smaller vessels, and in five minutes more, the large arterial and venous trunks, were dilated, and the blood, changed to an arterial red colour, moved very slowly, and in seventeen minutes more was nearly stagnant. In another case, an incision made in the web of a frog's foot in *two days* dilated the capillaries, and the blood, having been changed into an arterial red colour, moved very slowly, and in some vessels near the incision, became nearly stagnant. In a third instance, vesicles were produced in the web of the foot by immersion in water at 110°: the blood moved very slowly, and the whole web was very red. These are probably intended to be placed amongst those instances, in which, from the acrid nature of the stimulus, debility of the vessels is supposed to be induced, without any previous excitement. How far they are fair examples of such a supposed

condition may perhaps be questioned. They are, however, for argument's sake, instances of dilatation, without apparent increase of momentum, and unconnected with predisposition. It must, indeed, never be altogether left out of view, that, as long as there is motion in the circulating fluid, or a dilatation, or increase of quantity in a certain known proportion, there will also be increase of momentum; thus, diminish the velocity ten times, and increase the quantity ten times, you reach the same degree of momentum, and all the intermediate steps are instances of increase. Ten multiplied by ten is one hundred, and so is one multiplied by one hundred. Now, Dr. Hastings shews, that in vessels "much "dilated,"* there is a slow oscillatory movement; that a propulsion is going on, into the dilated capillaries, of a new volume of blood by the left ventricle;† and that the dilated state of the capillaries in inflammation is kept up by the impetus with which the blood is propelled by the heart and larger arteries.‡

The question before us is, whether inflammation can occur in cases, in which there has been no increase of momentum. In speaking of this latter condition, it has, by Dr. Parry, been considered as a cause, "in certain distinct cases," of the series of phenomena constituting the disease, which phenomena vary at different periods of inflammatory affection. The existence of the acute stage, according to the same author, is "not to be determined from the "mere circumstance of duration, which will vary in

* P. 85. † P. 110. ‡ P. 91.

"conformity to the state of the constitution and part, "and the coexistence and force of the causes of pro- "duction.* From the different actions or affections "in the different stages, it has been usual to employ "the expression, weak inflammation, a kind of chro- "nic state of increased fulness, to distinguish it from "the acute."† He is farther, of opinion, that it is "the expression of what in reality are nearly "opposite states in that series of phenomena, which "is called Inflammation, that has given occasion to "the assertion of the different microscopical appear- "ances of quickness or slowness of circulation, *either* "as occurring at different periods of the disease, or "as *produced by different external causes.*" In this latter clause, it seems not only to be admitted, that, in certain stages of the malady, a slow state of the circulation may exist, but that, by certain external means, the usual period of that stage may be anticipated. Neither of these occurrences, however, seems to have been considered sufficient to overturn the general doctrine of increased momentum, to which they may furnish exceptions. It cannot be denied, that, particularly in certain cold-blooded animals, an approach to a sudden suspension of motion, together with an entire change in the colour and consistence of the blood, may be induced by the application of certain violent stimuli, or by certain disintegrating processes. Whether the condition thus induced be a hastened step to that state of local disorganization, to which, in fact, the inflam-

* Elem. p. 72. † Elem. 111.

matory affections have, at all times, a primary tendency, and which, in common circumstances, the result of the slower process of increased momentum as a preceding link in the chain of causes, in the present case is effected by accelerated impulse, of a kind less palpable as to amount and duration; or, that such change, which is probably unattended by some of the constituent symptoms of inflammation, as increased heat and throbbing, be really equivalent only to certain usual consequences of the disease, which cannot strictly be comprehended under a definition of its essential nature.

It has already been observed, that the changes thus established appear to arise rather from a sudden alteration in the blood itself, than from any of the usual effects of slowly progressive inflammatory action, and rather from certain mechanical or chemical influence on the vessels, than from any immediate and direct modification of their vital contractility, however readily we may admit the possibility of such an occurrence. These changes in the blood may consist in the solution or separation of the colouring matter, and a new play of affinities among the internal parts, by which the constitution of the blood is entirely altered, and its relation to the containing vessels changed; thus producing that dilatation, which, as a nearly contemporaneous effect, has been assumed as the cause of the visible changes, and characterized as the product of debility. In considering the possibility of this sudden effect, it never can have been intended to include, as a fair example of the spon-

taneous inflammatory action, such as Dr. Parry illustrates in the specific instance of gout, the violence done to parts by laceration, nor the injuries of decomposing chemical agents; any more, than it can, in propriety, have been attributed to him, to deny the possibility of inflammatory symptoms from cases of sudden accident Dr. Parry's reasoning can, in fairness, bear no reference to these cases, to which almost all the arguments apply, of the writers who adopt the conclusion of debility. In admitting causes as operative towards inflammation, which act on the local tonicity of the vessels; in limiting the increased momentum strictly to the seat of inflammation, and excluding general impetus; while, at the same time, the effect is not made dependent on the duration of such increased momentum, we might possibly include, and explain, such cases of apparently sudden effect, even in conformity with his own doctrines. At all events, Dr. Parry professedly excludes this passive inflammation from his observations; considering it either as a remote consequence of the state essential to inflammation, or as one too dissimilar to the usual process to warrant its arrangement under the same head. In one degree of chronic inflammation, it is his opinion, that excessive momentum has, in a great measure or quite ceased, and that the over distended vessels are left to the free exertion of their tonicity, &c.* What is meant by this condition appears evident from Dr. Hastings's exposition; which also shews us that the opinions of Dr. Parry remain unaffected by

* Elem. p. 157.

his observations. He describes passive inflammation* as "a common termination of acute inflammation;" which Dr. P. not only admits, but adopts as the ground of difference between such condition and that which he is describing. The state of passive inflammation is, perhaps, an affection of the capillaries alone, not of the arteries and veins as in the other cases. It is, therefore quite distinct, also, from the inflammation of Dr. W. Philip, from whose definition† Dr. Hastings materially deviates.

The preceding remarks do not profess to unfold the mysteries either of the circulation, or of its deranged condition, inflammation. It has been attempted to reconcile some of the opinions of Dr. Parry with the phenomena of both states, as well by actual experiment, as by a consideration of the objections, practical as well as speculative, of the writers who take a different view of the subject. The author is neither insensible to the numerous weak points which his defence presents, nor of the difficulties which still, in this abstruse matter, remain to be surmounted, both in determining the precise relation of causes, and in the accurate observation and arrangement of the facts themselves. For the consideration of fresh objections, and, perhaps, for the extension of the basis on which further speculations may be built, an opportunity may possibly again present itself, and he will not willingly decline a continued investigation of this interesting subject.

* P. 104. † P. 76.

In the mean time, the following observations, which apply as well to the irritability of the vessels, as to the nature of inflammation, appear to him decisive in favour of the views here taken. They seem, among many other arguments, to prove the want of influence of the coats of vessels in the production of the states of circulation and inflammation; which are, therefore, not ascribable to any direct vital action alone, but to alternatives, still compatible with, and assisted by, the admitted vital powers of the whole vascular system.

Observation I. Pressure being made on the leg of a frog, so as to interrupt the action of the heart, the retrograde or oscillatory motions could, by such pressure, be commanded for any length of time; an effect which could equally be produced by altering the level in which the web was placed. The blood might, in fact, be poured backwards or forwards, or both, with any degree of rapidity; or its motion could be altogether suspended, by an adjustment of the parts, such as would have place, with similar effects, in any hydraulic apparatus. Now, it is conceived, that, in many of our experiments, in which the slightest variety of position would produce most important results of this kind in the minute vessels, such a cause is constantly acting, and, among many others, would be productive of the irregular circulation which we so often discover. It is farther believed, that under any influence constantly proceeding from the vessels themselves, according to the doctrine of those who maintain the vascular irritability,

such an effect could not be produced; and that, at all events, the assumed cause is unnecessary to the production of the manifested effect. It might also be supposed, that the function arising from such a presumed power might be carried on even in the forced circumstances of our experiments, as, under all varieties and changes of position, it is continued in the living system, without injury or impediment from the opposed hydraulic influence of gravitation: an event which does not take place.

II. But the same vessel, acting, as is supposed, by a vital contractility of its fibres in the direction of its circumference, can admit, at the same instant, but one action; it cannot, under this view, circulate blood in two directions. Among many other varieties in the instances of deranged circulation, the present author has, however, seen two streams of blood within the same portion of a vessel, one of which, at the same instant, flowed backwards, while the other flowed forwards: a fact, which appears to him, in the cold-blooded animals from which all the important inferences are drawn, entirely subversive of the doctrine of vascular influence, in the common process of circulation. The fact here mentioned might perhaps be more properly described, as a single stream divided into two parts, which circulate in opposite directions.*

III. Inflammation is said to arise from debility of the vessels, and the well-known changes which occur, are attributed to such debility; while it is also asserted,

* Plate, fig. 1.

that the blood recovers its natural constitution as soon as it gradually escapes into sound vessels. Under this supposition the blood is not, as is generally assumed, stagnant, but either this condition has not existed, or a change has already commenced, which admits such motions. Without dwelling on this apparent inconsistency, according to an observation made during the progress of experiment,* one part of an artery has been occupied by red, homogeneous, stagnating blood, and another side of the same vessel has contained the usual natural blood in full circulation. This fact seems incompatible with debility of the vessels, as a cause of the change in the blood; since there appears no reason why all the blood which flowed into the debilitated vessel, acting in the direction of its circumference, and not longitudinally, should not equally have ceased to circulate, if the effect really arise from the vessel; and if the previous change had not been made, as the author supposes, on the blood, and not on the vessel.

IV. But it is said, that debility is produced by sudden over-excitement of the vascular tunics, which can no longer contract, and which, therefore, cease to circulate; and that a change is produced in the blood by this debilitated state of the vessels. But, on some occasions, a conversion of the blood into the usual inflamed state takes place in certain arterial or venous branches only, though the whole web had been equally touched by the stimulus, and equally exposed to the debilitating cause. Now, all the fresh

* Plate, fig. 5.

blood which flowed into these vessels, and into the neighbourhood of the changed blood, underwent no alteration in its appearance. It seems natural to expect, that, under the same circumstances, the same cause should operate towards the same effect; and that the blood flowing into vessels, which had been exposed to the cause of debility, should have become affected as the first. At all events, the conclusion, that the change was on the blood, and not on the vessels, is quite as safe as the opposite deduction, and of course leaves all arguments drawn hence, towards the influence of the vessels, without proof.

V. That the power of the capillaries to assist in carrying on the circulation is, in fact, inferior to that of the larger vessels, if a proof is to be drawn from the diminished or accelerated motion in these tubes, seems evident from the fact, which has already been noticed, that, in almost all cases of derangement, productive of changes in the three systems, the capillary motions are suspended sooner than the rest. In much the larger proportion of instances, while the arteries and veins are transmitting or moving their fluids, the smaller vessels are a blank and inert mass; a state which will continue for a long time, the colourless fluid being either stagnant, or the vessels empty; while the oscillation in the veins will not restore this circulation, and the retrograding current in the arteries seems to have acted in withdrawing their contents from this minute series.

VI. Increase of velocity itself, which, on the application of a stimulus, is assumed in proof of the influ-

ence of the vascular tunics, is, in numerous instances, an effect unconnected with any contraction of the vessels. It arises, in fact, under circumstances, which rather give occasion to dilatation than to this state. In suspending the blood's motion, by an interruption to the influence of the heart from pressure, the state of the vessels is certainly not apparently altered; but in restoring the current, and with it often a greater degree of velocity, some dilatation of the vessels seems to take place, unless here, as has before been suspected, a deception arise from the vessel being more completely filled, than in its former state. At all events, there is no contraction, upon which the opposite theory depends for its support.

VII. According to frequent observations made by Haller, and which have already been noticed here, the vessels in cold-blooded animals are, upon many occasions, imperfectly filled with blood. Certum est dari arterias plenas, semiplenas, et inanes. Centies id vidi.* His decided experience on this point affords a powerful argument against any influence of the vascular tunics, in regulating the course of the circulation, or in producing the effects assigned to them in inflammation. The repeated observation, confirmed by Whytt, may also again be cited,† that, under certain circumstances, effused blood possessed all the varieties of motion, even against gravity, which in the vessels have been attributed to the direct

* Op. Min. i. p. 80. Exp. 75. p. 119. Exp. 217.

† P. 121. Exp. 222, 218.

influence of their coats; a fact, which, if correctly stated, is alone sufficient to refute the doctrine of irritability, and its connected debility, as deducible from the phenomena of these irregular movements.

VIII. That the condition of the blood is not dependent on the state of the vessels; in other words, that the fundamental position is erroneous, which asserts that the globules disappear because the vessel is debilitated, and therefore cannot contract, appears from many proofs, and from none more palpably than from the common facts of ossified arteries, in which the blood is not changed, but flows for years without difference in quality, notwithstanding the inadequacy of the vessel to the supposed function. If this state does not extend to the capillaries, yet it is admitted that the changes in the blood do extend to the arteries and veins; and, in either case, such a state of the vessels disproves decisively their assumed irritability, as a property necessary to the circulation of the blood, and the possession of which is the basis, on which is founded the doctrine of debility. If the diminution of the vital powers may be such as no longer to resist the tendency of the constituent parts of the blood to enter into new combinations, and to suffer chemical changes, these instances, in which the contraction of the vessels is entirely prevented, seem quite as efficient to the production of such an effect as any that can be adduced.

IX. That dilatation is not, in the instances adduced, always an effect of debility, is clear from the experiments themselves, which are brought forward in

proof of the contrary position. One of the evidences of the irritable nature of arteries, and therefore, their constant implication in the function of circulation, is the motion called oscillatory, which can, however, it appears, also, occur in arteries, "much "dilated."* It is evident that the movement, which can take place under the directly opposite condition of the vessels, cannot be the effect of their contraction. It may, on the other hand, be shewn, from the same authority, that the blood occasionally moves more slowly under contraction itself. "The blood in the "arteries moved slower, and their contraction in- "creased."† It is, then, in both cases, proved, by the experiments themselves, that the vessels have no concern in the phenomena supposed to be dependent on them.

That permanent dilatation of the capillaries, and a slower motion of the contained blood, may be induced, without the application of causes supposed to give rise to debility, appears from an experiment in which the Liquor Ammoniæ contracted an artery, and, in proportion to the contraction of this vessel, occasioned the dilatation of the succeeding capillary vessels, which had not been touched by the stimulus, and whose dilatation continued, though the other vessel recovered its natural state.‡

X. That in the capillaries there is no action, whether that be called contraction, or whether it be considered contraction and dilatation, appears evident from the circumstance admitted even by the able

* Hastings, p. 85. † P. 52. ‡ P. 51.

writer,* who contends for an alternate state of quiescence in the larger arteries, that in these vessels the stream of blood is uniform, which, under a direct influence, such as would result from either of the conditions above described, could not possibly exist. If, according to the evidence of sight, also, they possess no such powers, the arguments founded on that presumption, with regard to the nature of inflammation, fall, of course, to the ground.

XI. With regard to the state, whether of contraction or of dilatation, of the vessels, under inflammation, in which the former is assumed as a proof of increased action, the latter of debility, the primâ facie evidence is certainly not conclusive, but, on the whole, is unfavourable to the supposition of diminished action as a cause of the symptoms which are discovered. It may be enquired, whether in the frog and other cold-blooded animals, on which the illustrative experiments are chiefly made, the character of the disease be precisely the same as in man, and in other animals with warm blood; whether, for example, the temperature may have no possible effect in modifying the rate of motion, and arresting the tendency to stagnation, which, in the former class, is maintained to be the character of the inflammatory conditions?

XII. That inflammation may arise from obstruction, and consequent coagulation and change in the blood, does not find an objection in the circumstance, that, under such obstruction, the blood always escapes

* Med. and Chir. Journal.

into anastomosing vessels; since, occasionally, the blood flowing into the neighbourhood of obstructed vessels does accumulate for a considerable length of time, and, without any motion of retrogression, becomes stationary.

XIII. That no universal argument in favour of the irritability of the vascular tunics can be derived from the motions of retrogression and oscillation, on the occurrence of interruption to the influence of the heart on the circulation, is also very manifest from the circumstance repeatedly witnessed, that, on such interruption, a sudden and entire suspension of motion takes place, which motion can only be restored by a restoration of the connection with the heart, and is not at all affected by the application of the usual stimuli.

XIV. In describing the effects of irritability, Dr. W. Philip says,* "By the vital muscular contractile "power, the extremities of muscles are made to "approach each other." In circular muscles, then, like those supposed to surround the vascular tunics, the fibres can but extend partly round the circumference, in order that there may be any exhibition of action, or that such changes may occur; and yet how can such action be reconcileable with the other positions, that the "firmness and transverse diameter "increase in a degree which bears a certain pro-"portion to the approximation of its extremities?" It is evident, that, in a set of circular fibres, if the extremities are to approach one another, the trans-

* Quarterly Journal of S. and A., vol. xiii. p. 97.

verse diameter of the fibre must be stretched, that is diminished, instead of being increased; and that any sort of approximation of the extremities to their own centres, or other than in the direction of the circle, would dilate, instead of contracting, the area.

It may, also, be observed, that the increased diameter of the muscle would probably counteract the sensible appearance of contraction, and a nearly uniform condition exist under all circumstances. Now we know that the effect of contraction is very palpable in the vessels. On these grounds, it may be inferred, either that the above is not a correct account of muscular action, or that the vascular changes depend on some other power than that of muscular irritability.

The subjects connected with this inquiry, which may still seem to demand a more particular examination, are the following:—

First, The cause of increased velocity in the circulation, independently of contraction of the vessel.

Secondly, An account of the exact changes in the blood, under the causes inducing the inflammatory conditions.

Thirdly, The facts of inflammation suddenly induced, without disorganization, and without evidence of increased momentum in any part of the series.

Fourthly, The facts with regard to dilatation, whether mechanically or otherwise induced, and of accelerated circulation, as inducing morbid

changes in the blood, analogous to, or identical with, those of inflammation.

Fifthly, A more exact distinction between the faculties of irritability and of tonicity.

With regard to the *first* subject, it must be evident, that, having endeavoured to prove that vascular contraction, such as it has been described, has no existence; and that, if it did exist, it could not produce the effects assigned to it; it is not incumbent upon us to shew how the increased velocity is produced, and that such extended inquiry would, on this occasion, be matter of curious speculation alone.

It has been said, that to seek any other cause for this effect than an immediate influence from the vessels, is, as in the case of Democritus and his figs, to go far off for what is to be found at home. To this petitio principii we have nothing to reply. If, however, it may seem opposed to probable reasoning, to believe that there can be another cause for such motions, the fault does not rest with us.

On the *second* head, it is very important to extend our knowledge, as it is the basis, also, of the third and fourth inquiries. The experiments of Messrs. Prevost and Dumas have given us some precise information as to the nature of the blood in health, which may serve as the ground on which many morbid effects may, perhaps, be explained, particularly the influence of stimuli in their relation to the visible changes which occur in the supposed state of inflammation.

According to the examination of these authors, the blood consists of the serum and the clot: the former varies in the same individual, and in different animals; the latter consists of the totality of the particles (corpuscles), and of some serum. The number of particles bears a *certain relation* to *the heat* developed by the vital action. In a frog the particles are fewer by nearly one half, than in man, or in the proportion of 690 to 1292, and the albumen and salts of the serum are as 464 to 869.

I.	II.
Serum	Corpuscles
Albumen and Soda.	(1) A central colourless Spheroid, and (2) A red membranous Bag.

I. When the albumen is coagulated, it shews the same white globules as are found in the red part. Spirit of Wine coagulates the albumen, by its affinity for caustic soda. Albumen differs in no respect from fibrine. The acids act in the same way on the albumen—they saturate the soda, and coagulate the albumen.

II. The central colourless spheroids are disposed to form aggregates or ranges, like strings of beads. They unite on the cessation of motion, and the colouring matter escapes. These are the *fibrine*. The colouring matter is easily divisible, but is insoluble in water. It consists of an animal substance (albumen?) and peroxide of iron.

It appears from the experiments of Mr. Hunter,* that an immediate contact of the air with the blood is not necessary to produce its known effects, but that its influence in occasioning a florid red colour was perceptible even through interposed membranes. It farther appears, that heat coagulates the blood.† According to the same authority,‡ rest and slowness of motion, as well within as without the vessel, are among the immediate causes of the separation of the red particles, and of a change of colour. In an inflamed part no such change is effected by stagnation, but the blood remains of the same florid hue as before.§ Upon these, and the lately recited facts as to the composition of the blood, we may enquire, what are the effects of heat, Liquor Ammoniæ, the Nitric and Sulphuric Acids, and other stimuli, on the constituent parts of the blood itself through the vascular structures; and whether by them any change can be effected in the albumen, fibrine, or alkaline salt. A total absence of influence from the continued application of these agents cannot well be imagined, and it is evident that all such influence furnishes an alternative to the supposed sole effect on the muscular structure of the vessels. If the white globule of the clot be fibrine, and if the fibrine and albumen be ultimately the same, and both analogous to the muscular fibre; it is not impossible that there may be some influence on the irritable nature of the blood, without seeking this in

* On the Blood, p. 62.

† P. 96. ‡ P. 29. § P. 68.

the muscularity of the tunic. The effect of the application of stimuli may be conceded; though the irritability be in the blood, and not in the vessels.

If it is true, indeed, as Dr. Hunter and Dr. W. Philip contend, that there is an entire analogy between the process of coagulation in blood, and the contraction of the muscular fibre; it is probable, that the same constituents are engaged in the same effects, and are affected by the same causes, though from difference of circumstances and condition, the effect may occasionally not coincide. In such case my argument is rendered highly probable; and while the supposed absence of contractile effect from the application of stimuli would disprove the presence of muscularity in the tunic, the coagulation induced would go to assert the direct influence on the blood. The effect of stimuli may as easily be on the blood, which we have seen is acted on by the air through an interposed membrane, and not upon the vessel, as upon the vessel and not upon the blood.

The more recent experiments of Dr. Scudamore are among the most interesting and instructive, which have yet appeared on the subject of the blood. From them it appears that an excess of fibrine is the circumstance which universally occurs in the highest state of inflammation; but that, in the earliest stages or commencement of increased circulation, "in the "first few hours of inflammatory action," this excess is not present. The highest proportion ever found in healthy blood has been, in 1000 grs. of the clot, 4.43 grs. and in a mean of eleven examples, only

3.72 grs. while the amount in what is called inflamed blood, has been as great as 20.53 grs.

Though, however, increased circulation from exercise in health does not seem, at first, to produce this condition necessary to inflammation, but on the contrary to take off the disposition to fibrine, and to increase the serum; and though, therefore, the mere increased rate of circulation does not, at once, induce the inflammatory state; yet as a continuance of what is here called disease, is required to give rise to the buffy coat, and as, unless the fibrine be in considerable excess, we cannot strictly call the previous state inflammatory; so it may also be, that increased circulation continued beyond a certain point may itself constitute the incipient disease, of which the consequence is a retention of that fibrine, which, under a natural and less hurried state, would have been distributed in a greater proportion to its congenial textures.

Allusion has elsewhere been made to the condition of blood which is assumed under such accelerated circulation in pregnancy and other states; and the example of healthy exercise, with its necessary corresponding repose, does not seem opposed to the hypothesis, which assumes the extreme case, as a cause of the effects which we witness in acute and general inflammation.

With regard to the effects of numerous chemical agents on the blood, Dr. Scudamore supplies us with many important facts. The solidification of the fibrine is, according to this author, the essential

cause of coagulation, which, in blood drawn, is prevented by Muriate of Soda and by the Liquor Potassæ. Here again, with regard to local inflammation, we may enquire, can these agents have no influence on the blood through the vascular tunics? By dissolving the fibrous or other constituents, and by thus diluting the blood, can they have any influence in producing an accelerated movement; can stagnation, or a tendency to this state, have any connection with a chemically induced condition of coagulation? Can the vivid red, occasioned by the action of the alkalies, become apparent by their external application, and have a relation to the brilliant colour which we witness in so many of these experiments?

A tendency to coagulation in the living vessel, from fibrine locally, and in excess, accumulated, seems to be a material circumstance in the inflammatory state; and if accelerated circulation does not, in many instances, so produce the effect, in other cases, under the extensive and important influence of predisposition, such a condition will occur, and give rise to the phenomena well known to constitute the disease called Inflammation.

Of the *fifth* head, the few following particulars may be noted, as indications of a difference in the faculties distinguished by the names Irritability and Tonicity. They appear, at least, a foundation for further inquiries into these distinctive properties of animal structure:—

Irritability, not excited, accumulates.

Tonicity not called forth is lost.

Irritability survives the life of other parts.

Tonicity does not so survive.

Irritability acts under stimulus alone.

The effects of Tonicity are most manifest under the absence of stimulus.

The highest degree of Irritability is said to be attended with alternating relaxation.

The highest degree of Tonicity is permanent contraction. They equally differ in other degrees; as Irritability can never permanently contract.

Irritability is independent of the brain, though it may be influenced through it, and by other stimuli.

Tonicity seems derived from the brain, and is not acted upon by stimuli.

Irritability has no necessary connection, in its seats, with the elastic power.

Tonicity and Elasticity are generally united for their destined purposes in the animal economy.

The actions of Irritability, according to certain recent experiments, are often not manifested in one or two hours, under the influence of stimuli, which are said ultimately to affect them.

The action of Tonicity is immediate. Remove the blood, or render it unnecessary, contraction immediately follows.

Irritability has no mean state. It exists only under excitement.

Tonicity has a mean state, and is manifested under the absence of excitement.

These distinctions, and the several subjects to which the reader's attention has already been called,* still open a wide field for speculative inquiry, and for experimental research. While the difficulties which they seem to involve, remain unexplained, we must still feel some uncertainty with regard to many points of practical importance, and of much interest, in the question of the circulation of blood, and of the phenomena of the vascular system.

* P. 118.

CHAPTER III.

DETERMINATION OF BLOOD.

WHATEVER question may be entertained, as to the novelty or importance of the subject of Determination of Blood, in its pathological and therapeutical relations, it has never been denied that this doctrine received a very full elucidation from the industry and original researches of Dr. Parry: and though it may scarcely be worth while to defend him against a charge of plagiarism, which has never been seriously advanced, or to maintain, what no one has disputed, the pure sources from which his conclusions were drawn; it might not be a difficult or unworthy task, under other circumstances, to shew that his views differed, in many essential respects, from those of his predecessors, and that they had a material advantage over the dark and imperfect hints of previous inquirers, as they formed the basis of an enlightened and efficient practice, and were illustrative of many connected effects, hitherto unobserved or misunderstood.

That an important and beneficial change in the treatment of many diseases resulted from the views adopted chiefly by this author, will not be denied by those who remember the state of medical practice half a century ago, and who witnessed the obstacles which were long opposed to Dr. Parry's opinions, as well by the deep-rooted prejudices of his patients, as by the no less adverse theories and example of many intelligent physicians themselves. His correspondence on these subjects, during a period of nearly forty years, forms an ample and sufficient commentary on the remarks of those who either consider the principles of recent introduction into his practice, or who incautiously seem to charge them upon him as not his own.

In considering the scope and tendency of the doctrines, which may now be designated as those of the author of the Elements of Pathology and Therapeutics, a main question arises as to the proposed extent of their application. Did their author adopt a principle, which he wished to extend to all diseases, as the basis of a Universal Pathology? He expressly states that neither his "Elements," nor the work of which it was the outline, was intended by him as a complete system of pathology, or as inclusive of all the diseases to which the animal frame is liable:* And, though he endeavours to shew, that almost all, or far the greater number of, maladies have the circumstance of excessive momentum of blood, as *one* important common *link* in the great and connected

* Elem. p. 321.

chain of causes, that is, of uniformly preceding phenomena;* he insists, at the same time, on the fact, that many diseases arise from a condition the very reverse of that which he attempts to illustrate in these instances; and specifies, as a subject for particular consideration hereafter, a series of phenomena, of which defective circulation forms a conspicuous symptom. He has likewise hinted at various effects,† different from mere sanguineous impulse, and many causes of disease, which cannot be comprehended under the head of excessive momentum.

But how far was he disposed to consider this particular doctrine as including a sole principle or origin of disease? A very superficial examination of his opinions will convince us, that he neither attempted to establish the primary cause of the various diseased conditions, nor even to approach a discovery of that, which will probably be for ever hidden from our view. The particular state, which he illustrates by so many examples, forms one link only in an extensive series of causal phenomena. "It is a symptom, in far the greater number of diseases, occurring at some point or other of that succession of antecedent circumstances, which constitutes the chain of causes.‡ In discussing the nature of proximate causes, it is evident that Dr. Parry neither believed this circumstance to be the origin of disease, nor disease itself to consist in this condition alone; and, though he did not think it necessary to prove that a proximate cause is only a single link in the chain, he endea-

* P. 298. † P. 275, 277. ‡ P. 298, 371.

voured to shew that it is the phenomenon most immediately preceding the state called disease, and that it is, therefore, not a part of the disease itself. He has also assigned other proximate causes to certain diseased appearances, and considered increased impetus of blood to be, in some specified examples, only a remote or exciting cause.*

In taking a survey of his most illustrative instances, is it possible to connect with them the opinion, that excessive momentum of blood is the sole circumstance of disease? Can it be implied, by any possible perversion of his doctrine of nervous disorders, in which the brain and nervous system are morbidly affected by a particular relation of the circulating system, that in this latter system is to be found the sole origin of such affection, and that the disposition and properties of the brain and nervous system are excluded from an agency in the production of the manifest effects? With Dr. Parry, the determination of blood was neither a universal pathology, nor exclusive as a cause, nor a sole and specific condition, of disease. He considered a disordered state of the whole or some part of the sanguiferous system to be the most obvious of all the deviations from health,† and, in numerous instances, the only tangible part in the great and connected chain of causation. He has supplied us with a list of those disorders, in which this symptom appears to be predominant, and, what is of more importance, a valuable or necessary guide for our practice. It is his opinion, that exclusively of those

* P: 276. † P. 4.

maladies which arise from the influence of mechanical causes, or from infectious or other miasmata, these are the principal affections to which the animal frame is liable.

Without entering into the novelty or merits of the doctrine, and without forming any prejudged estimate of its practical importance, it will be for those who oppose him to prove that, in these particular cases, his opinion is incorrect. But has it ever been shewn, that, in the adduced instances, increased impetus, momentum, or determination of blood, does not form one essential link in the series of phenomena or causes, and, that if this link be wanting, whatever, may have been the previous diseased condition of the seats of affection, that disease will not follow, the symptoms of which are the present marks of our observation, and the immediate rule of our treatment? Till this be done, his opposers have done nothing; and can neither be prepared to assert that his system is essentially erroneous, nor to disprove the accuracy of the principle by which his practice was mainly regulated. The question is certainly comprehended within very narrow limits. It is not, whether there be other links in the chain of causation, nor whether there be other diseases not liable to this arrangement, both of which facts he readily admitted; but, whether in such diseases this link occur, deserve the attention which has been bestowed upon it, and may, in any respect, be adopted as a guide for practice. It is enough for Dr. Parry to have attempted the demonstration of these positions.

To these effects of locally increased momentum occurring in disease, two preceding circumstances, a local or a general cause, may give rise. Local determinations are, according to this author, frequently connected with the state of the vessels alone,* whether that state be a preponderance of the elastic over the tonic powers, weakness or strength, diminished or increased action ; and they are, therefore, not necessarily connected with increased general momentum, even though this be of considerable duration.† In his opinion, the capacity of increased dilatation of arteries, and a consequent local fulness or momentum, independently of increased action or any observable change in the circumstances of the heart, is proved by many facts. Though, however, the afflux of blood to particular parts is often connected with the condition of these parts, and vessels are consequently subject to undergo the changes above stated, from causes acting immediately on themselves, "they are still more liable to them, through the "medium of the action of the heart."‡ In what cases they depend merely on excessive local momentum, and in what there is the coincidence of increased action of the heart, it may be difficult, in every instance, accurately to decide.§ That, according to Dr. Parry, all diseases were not, strictly speaking, local, is decidedly pointed out by the common illustration which he adopts of the character of morbid determinations, taken from the phenomena of ephe-

* P. 51. † P. p. 45, 46, &c. § P. 322.
‡ P. 21.

meral fevers. It has, indeed, been frequently, and with injustice, urged against these doctrines, that they were too much disposed to exclude local conditions, and to attribute the symptoms of disease to the influence of general momentum, and thus to impute its origin to general, and not to local, causes.

But, under changes, whether from topical or general affection, how does it happen, that, in some constitutions, the morbid effects, sooner or later, occupy particular seats? and why are some persons always locally affected by general causes, while other escape, notwithstanding their continued application? Independently of sudden and mechanical causes of affection, and the effects of infection and miasmata, local determinations are universally connected with certain unknown and spontaneous conditions, which predispose the constitution, or the part, for the changes which are to take place. We are, according to Dr. Parry, constantly induced to have recourse to the supposition of a predisposition, or previous tendency, to particular diseases in the persons liable to be affected. So again,* with regard to those changes of determination, which have been called Conversions of Disease, and of which every experienced practitioner must have noted numerous examples, we must still have recourse to predisposition.

As a summary of his knowledge on this subject, Dr. Parry candidly states† that, whether local morbid determination occurs from causes acting primarily on the part, or secondarily, through the medium of

* P. 48. † P. 51.

excessive general impetus, it is equally difficult to explain the process by which it is effected. Such, indeed, was the philosophical precision which he applied to the investigation of facts, from which important practical conclusions were to be drawn, that he disclaimed, as unintelligible and illegitimate, a supposed "Increase of the Powers of Life," as a cause of the extraordinary afflux of blood to particular parts. "To these words of Bichât," says Dr. Parry, "I am utterly unable to attach any definite ideas. "All which, in such cases, the evidence of our "senses teaches us, is more or less of the states "already described, from whatever cause that state "may arise." We may shortly have occasion to observe, with how little consistency and benefit a reliance is placed upon these invisible properties, when, by another intelligent physiologist, the final cause of the same effects is placed in the opposite condition, a decrease of these very powers of life.

Having taken a hasty survey of Dr. Parry's opinions on the subject of determinations of blood, we are prepared to enter into an examination of the principal arguments of some of those authors who profess to differ from him, and who have taken the trouble to question his accuracy on certain essential points of theory, as well as of practice. An ample and detailed commentary on these doctrines has lately appeared in an "Exposition of the Principles "of Pathology, and of the Treatment of Diseases, by "Dr. Pring;" a work, which, however little value may have been attached to its practical results, re-

flects the highest credit on the mental capacities of its author; and which, however it may excite the envy of less brilliant minds, supplies, at the same time, the consolatory reflection, that a limit is necessarily placed to human inquiry, and that man, gifted as he may be, cannot entirely remove the veil which surrounds the mysteries of life and nature. It must, at the same time, be remarked, that, although the friends of Dr. Parry hail this work, as treating the subject of its criticism with the utmost liberality and candour, and consider it as confirming, in every material respect, the most important of his doctrines, they cannot altogether view it as a fair or accurate exposition of his opinions or principles. That the abstract supplied by Dr. Pring is scarcely applicable to Dr. Parry's doctrines, will sufficiently appear from the following passage, which, on many accounts, demands a full insertion:—"It is superfluous," says Dr. Pring, "to make any allusion to those by whom "this doctrine of determination of blood has been "merely entertained: it is indebted for its improve- "ment, perhaps for all the perfection of which it is "capable, to the late Dr. Parry, whose works on this "subject are written in a spirit of true philosophical "research, which has been equalled by few of his "contemporaries, and has rarely, in medicine, at any "period, been surpassed; yet, by this free tribute to "the memory of a man of clear and strong understand- "ing, I would not be understood to acquiesce in his "opinions, which appear to me erroneous in some "essential respects. But although those of his opi-

‘ nions which are proposed as general doctrines or
“ principles, appear to be in a great measure erro-
“ neous, they are nevertheless illustrated by so ex-
“ tensive an experience, and by observation of so
“ much worth, that he who is not acquainted with the
“ results of this experience and observation, will fall
“ short of the most enlightened views of medicine,
“ which are furnished by its present state. More
“ than thirty years ago the attention of Dr. Parry
“ was, I believe, first particularly directed to the
“ importance of the circulation, by a case of hysteria,
“ in which, from an obvious determination of blood
“ to the brain, he was induced to try the effect of
“ pressure on the carotids, with a view of diminish-
“ ing the quantity of blood sent to this viscus. The
“ symptoms were diminished or suspended during
“ the compression of the carotids, and repeated sub-
“ sequent experiments of the same kind led to the
“ conclusion, that the cause of spasmodic diseases,
“ proceeding from disorder of the brain, consisted in
“ a determination of blood to this part. Thus far
“ there is nothing new in the theory. But from the
“ obvious connection of the circulation with cases of
“ this character, the Author of the Elements of
“ Pathology was induced to seek for a similar con-
“ nection in other diseases; and as there are few
“ diseases in which there is not more or less of
“ disturbance in the circulation, so it is no wonder
“ that this disturbance or irregularity being generally
“ remarked, a general importance was assigned to it,
“ equal to that which it at first obtained in some

"solitary examples; and that which was in the begin-
"ning little more than a medical observation, per-
"haps an accidental one, was at length systematized,
"and furnished the basis of an universal pathology.
"It is asserted, by this pathology, that every disease
"is local, or that it has, at least, a local origin;—that
"the cause of such local disease is that the vessels
"of its seat contain a preternatural quantity of blood,
"giving rise, in conjunction with the impulse of the
"blood derived from the heart, to different degrees
"of momentum, producing, and corresponding with,
"the varieties of the disease;—that, as this circum-
"stance is an universal accompaniment of disease,
"so it is universally the cause of all those phenomena
"which the seats of disease exhibit;—that deter-
"mination of blood is either simple or inflam-
"matory;—that functions may be disordered by such
"determination, which ceasing merely by the vessels
"assuming their natural capacities, the disorder of
"functions occasioned by it may also cease;—that
"the ending of determination of blood is at other
"times in secretion; and that secretion is the spon-
"taneous mode of relief or cure, when determination
"of blood, whether simple or inflammatory, occupies
"a membranous or secreting structure;—that mere
"irritation, disorganization, diminished natural se-
"cretions, increase of natural secretions, preternatural
"depositions of every kind; in short, all diseases,
"functional and organic, are the children of one
"common parent; and that determination of blood
"to the seats of these phenomena is the cause of

" them all. The doctrine farther states, that a deter-
" mination of blood to one seat might cease upon the
" occurrence of a determination to another; and the
" Author calls this the Cure of Disease by Conversion;
" thereby expressing, in other language, the old
" observation in medicine, that one disease sometimes
" cures another."

It has been necessary to make this long quotation, in order that a fair judgment may be formed as to accuracy of its remarks, in connection with those opinions and principles of Dr. Parry, which have been already given nearly in his own words. It is scarcely worth while minutely to inquire into the justice of the observation, that the doctrine which assigns the cause of nervous disorders to determination of blood is devoid of novelty; but we might properly ask, for the sake of information, where it has been shewn, " that nearly all the modifications " of the disorders, usually so called, originate in ex-
" cessive momentum of blood in the vessels of the
" head;" or where a practice, regulated by such a view, had been previously adopted? It may, then, perhaps be time for a more accurate investigation of this particular objection. In the mean while, it has been pointed out by Dr. Parry himself, that all nervous disorders do not arise from such a cause;* that other causes may produce a similar effect; and that, though excessive impetus of blood is evidently most often productive of the effects, and is frequently a concurrent cause, of nervous disorders, of which the imme-

* P. 275.

diate cause "is in the brain itself,"* great disturbance of the mental functions, and even the total death of the brain, may arise from other causes. It is, indeed, clearly deducible from the passage above quoted, that when Dr. Pring asserts the want of novelty, he does not charge upon Dr. Parry the want of original discovery, since he admits that the observation which was the basis of all his subsequent arrangements "was, perhaps, in the beginning an "accidental one."

It appears, from the view already taken, that it is not in accordance with Dr. Parry's doctrines to assign a local origin to every disease, though many local affections, arising from predisposition, may be the consequence of continued general action, such as occurs in fevers and inflammatory disorders; neither does it necessarily follow, that the vessels of the seat of such disease contain a preternatural quantity of blood, or that the impulse of the blood from the heart alone is required for increased local momentum. There may be increase of momentum from increase of velocity alone, and that state may result from local causes.† The circumstance of preternatural quantity being, however, admitted, it is never asserted, by Dr. Parry, to be a universal accompaniment of disease; on the contrary, many diseases occur under the reverse conditions; and, by no possible construction, can it be deduced from his doctrines, that such a state is universally the cause of all those phenomena which the seats of disease

* Pp. 235, 236, &c. † P. 279.

exhibit. It is no where asserted, or implied, that all diseases, functional and organic, are the children of one common parent, and that determination of blood to the seats of these phenomena is the cause of them all. Neither is the term, Cure of Disease by Conversion, an invention of Dr. Parry; least of all, does he arrogate to himself, as implied in the above extract, the supposed merit of having discovered this natural remedial process.

It must, indeed, appear very evident, that Dr. Pring's exposition of these doctrines is an imaginary one, and that his consideration of the principle,* first, that determination of blood exists in every disease, and, secondly, that it is the cause of every disease, is the consideration of an hypothetical case, which need not have occupied his valuable time. The argument does not stand, as he places it, in the following syllogistic form: "Since most diseases are "accompanied by determination, we infer that all are." Admitting it, however, to be the case, for the sake of argument, let us examine the proofs adduced by Dr. Pring, that a determination of blood does not exist in all cases, though, it might do so in ninety-nine cases out of a hundred. He argues against the principle, because, in the hundredth, an inspection after death proves that no such determination had taken place, even in diseases connected with symptoms, "for the occurrence of which a "determination of blood has been most peremptorily "insisted upon."† It may, however, be permitted

* Exposition, p. 148. † P. 149.

me to ask, whether the state, after life has ceased, necessarily coincides with that which existed during life; or whether, under no possible suppposition, the ceasing of a vital action may occasion the cessation or modification of conditions dependent on such vital action? Farther, whether, under no equally possible supposition, the state of local fulness may not have been removed, and this previous symptom have been entirely superseded, by the very affection which induced a fatal disease? Dr. Parry has, upon probable grounds, endeavoured to shew, both that effusions, where they have existed, remove the previously diseased state, and that spasm and convulsion are effects, or processes of re-action, by which preternatural local fulness is relieved. Although, therefore, the severest symptoms have existed during life, without any evidence being afforded by examinations after death, of a preternatural determination of blood, it does not, in any respect, follow, that such a state was not that which preceded the described effects. It might, perhaps, be argued, on the other hand, that, in proportion to the severity of the symptoms, would the absence of this appearance be more decided, and that preternatural fulness of vessels should only be manifest in cases where this salutary effect does not take place, as it occurs in Hysteria, Epilepsy, Tetanus, and other similar disorders.

But Dr. Parry's doctrines do not even require the presence of preternatural vascular fulness or congestion in such cases, since even with regard to epilepsy and nervous disorders, he only makes exces-

sive impetus or momentum of blood, acting on the medullary substance of the brain and nervous system, the concurrent cause of the symptoms.* But, excessive momentum may exist without local excess of blood, and even under a diminished quantity; while, at the same time, this impulse, whether dependent on the heart, or on local conditions, might itself, perhaps, prevent that accumulation, which would occasion the appearance of preternatural fulness, and even be a cause of an unusual emptiness in one part of the series of vessels. It has, indeed, not only been admitted by Dr. Parry, but given in illustration of the principle of simple determination without effusion, that in apoplexy, one of the most marked cases of such determination, instances may occur of recovery without such an event, and which therefore suppose the early removal of the symptoms giving rise to the diseased conditions; so that, even in this case, under accidental death, no such vascular fulness might be discoverable. It will be recollected, too, that in cases of simple determination, not amounting to actual inflammation, it does not follow that the capillary system should have indications of turgidity or fulness. This may have occurred in the larger systems of vessels only, or in the branches connected with them. Now, according to the experience of many physiologists, though the fact is not accurately stated, these vessels are entirely deprived of their blood by spontaneous processes occurring after death, which, especially if the capillary system have an influence

* P. 236.

in such derivation, would be no less operative in the present instances.

It does not, therefore, appear a sufficient argument against the existence of determination of blood during life, that, in the cases cited, there is no appearance of such a state after life has ceased. Notwithstanding his argument, Dr. Pring is, however, himself disposed to admit, that, in these instances at least, such a determination really does exist.*

But it is possible, as has already been noticed, to extend the illustration even to inflammatory affections, in which the appearance of preternatural fulness, on dissection, might be supposed least of all questionable. We learn from the "Elements of Pathology,"† that in many diseases, of which it is a concurrent cause, as in dropsy, there is, on the occurrence of this symptom indicating the sensible disease, no evidence of vascular fulness, and that excessive momentum ceases, as soon as its morbid effects, whether on the membranes or on the nervous system, are complete, and the over-distended vessels are left to the exertion of their own tonicity. It may also be remarked incidentally, that, as it is not essential to the proof of determination existing during life that there should be local evidence of such a state after death, so neither is it against the reality of the state in life, that the principal vessels connected with such circulation seem rather to indicate the presence of an opposite condition. Dr. Parry has shewn, that fatal sanguineous apoplexy may occur, even though

* Exposition, pp. 171, &c. † Elements, pp. 118, 381.

there has been no unusual fulness of the carotids, and though the face was pale before and after death, with a generally low circulation.*

Dr. Pring's argument does not prove, that determination of blood is not an universal accompaniment of disease.†

But is this determination of blood a primary cause of disease? Dr. Pring asserts, by implication, that the Author of the Elements constitutes it such a cause; while himself, on the contrary, maintains this state to be a consequence only of previous disease. Since the equal distribution of blood depends upon a healthy state of the powers by which it is moved, and on which its distribution depends, "if a part "obtain an undue quantity of blood, this excess of "blood cannot be the cause of the disease, because "disease itself must precede its occurrence." The inference drawn by Dr. Pring does not appear to be strictly and universally correct, as there are numerous cases of disease locally arising without any evident morbid predisposition, and under circumstances distinct from morbid condition. Where, among other instances, is the absolute necessity for diseased predisposition in parts affected by determination of blood under the agency of cold elsewhere applied? That previous local disease is not always necessary for morbid determination of blood, is, indeed, admitted by Dr. Pring himself, when, in discussing the causes of cerebral congestion, an accelerated action of the heart, by which blood may be urged

* Pp. 279, 287. † Pring, p. 193.

into the arteries faster than it can be returned by the veins, is placed among the causes of such condition, and is considered as an analogy applicable to the case of all the other viscera. Now, that accelerated action of the heart itself may occur without disease, requires no proof; and disease arises as a consequence of a healthy circulation, accidentally deranged, and under circumstances indicating no previous tendency to a diseased state.

But what reference has it to Dr. Parry's position, to shew that some unknown and inscrutable condition of disease precedes the several effects? Does the one disease precede, which he ascribes to the presence of an undue quantity, or velocity, of the circulating fluid? Certainly not. Whatever may be the morbid state of the brain, or of the vessels, the symptoms of the disease which he is describing, whether hysteria, epilepsy, or inflammation, will not occur without this particular change which he describes. It is not, therefore, accurately stated that the disease precedes, since, in fact, it follows the indicated conditions.

Again, has Dr. Parry any where endeavoured to prove that this sanguineous impulse is a primary link in the chain of causation? Certainly in no one instance. In cases of local affection from excessive action of the heart, this state of the heart may be a previous morbid affection; and in every example of local fulness, he himself assigns the origin of such disease to a pre-existing state. It is sufficiently manifest from all his writings, that the presence of an excess

of blood is not required or maintained by him to be prior to other phenomena of disease.* In speaking of epilepsy, he expressly alludes to other primary causes, distinct from excessive impetus of blood, and cautiously employs the expression, "Whatever its "primary causes may be."†

Determination of blood, then, according to both the authors in question, is generally the consequence of the assumption of a state of disease. It is part of a diseased state, which it helps to establish; and may be only auxiliary to the other causes involved in the condition of disease in deciding its varieties and duration.‡ That, in the examples adduced by Dr. Parry, this condition is a link in the chain of causes, and one of universal occurrence, is not yet disproved. How many other links may complete the complex chain of existence, under the operations of health and disease, may not, perhaps, be so easily told, and it may, possibly, be as little necessary to inquire.

To argue against Dr. Parry, that he considered the state of disease as acknowledging one cause alone, and that he asserted it to be a simple and uncomplicated state, "independent of the properties which "concur to produce the structures to support their "life, and to maintain their health," is to take a very imperfect and erroneous view of his doctrines. It was, in fact, the high importance which he attached to other parts of the frame, and to the nervous system in particular, and the attention he had for

* Pring, p. 153. † Elements, pp. 217—8.

‡ Exposition, p. 154.

many years exclusively paid to this latter important part of the animal economy, which, as has been accurately stated, led him to an understanding and extended application of the facts, connected with the influence of the circulation in health and disease. Having ascertained the frequent existence of this link, and being satisfied that the phenomena of the nervous system were less within the controul of human means, and that the properties of life were inscrutable by human powers, he discarded the theories which, in his own knowledge and experience, led to the most injurious practical results; and adopted a doctrine more consistent with facts, and of more evident and valuable application to the purposes of human life. It might, indeed, as safely be contended, that in the application of the powers of condensed steam, of compressed heat, or of ignited hydrogen gas, their discoverers necessarily exclude from consideration the complicated mechanical arrangements, by which alone these several principles can exhibit their stupendous properties, as charge upon the Author of the Elements the belief, that this sole principle, the determination of blood, was explanatory of all the manifold circumstances discoverable in the simplest, as readily as in the most complex, processes of deviation from the usual and admitted standard of health.

That the causes of disease are complex, or that their phenomena cannot be reconciled with the supposition of a solitary cause, Dr. Pring endeavours, somewhat unnecessarily, to shew from the state of disease itself, which designates conditions of the

properties involved in the disease distinct from the degree of determination.* All this will readily be conceded; and yet the question may again be repeated, whether, without this very occurrence of irregular determination as "one concurrent cause," any of the specific effects would have followed? It has, indeed, little reference to the question at issue, to investigate what particular modifications in disease may arise from all the diversity of relations, which must exist in a complex healthy frame, or what under the new conditions dependent on the assumed state of disease.

To say that the doctrine of determination is inadequate to explain facts, is to assert only what is universally admitted.† As the several parts of the animal frame vary in their functions, so do they also, according to Dr. Parry, in the quality and order of the phenomena which constitute their diseases.‡ Besides those particular conditions which he has pointed out, there are "various other processes which "attend the animal frame in health and sickness." Inflammations are varied by local connections. Specific actions occur in various organs destined to such purposes, under the operation of various common causes. The influence of cold, and of heat, and of miasmata, will act, as that common intermediate agent the blood acts, and induce, in parts predisposed, certain determinate complaints involving numerous properties. It is, however, not in remote conse-

* Exposition, p. 157. † P. 159.

‡ Elements, pp. 80, 142.

quences, that we are called upon to infer a resemblance in the condition of certain diseases, but in that particular effect which is predicated as an early and frequent occurrence. It cannot easily be disputed, that, where cases begin to digress, this is not to be ascribed to circumstances in which they were alike, but to some particulars in which they differed.* Dr. Parry has himself taken pains to prove a diversity of effects from the same cause, according to diversity of textures, and specific predispositions. In admitting the diversity of diseases, does he not also admit, that peculiarities of effect occur from a different disposition of the organs, from the agency of properties which are independent of the blood, and peculiar to particular cases.† Here, as in many other instances, it cannot, indeed, but be a matter of regret, that the views and opinions, which Dr. Parry had reserved for a more detailed history of diseases, were never prepared for the public, and that we must be satisfied with the concise outline supplied by his Elements of Pathology and Therapeutics.

Notwithstanding an apparent opposition to his doctrines, it may perhaps be correctly stated, that the Author of the Exposition admits all that can be required by the most zealous advocates of Dr. Parry. "It is," says Dr. Pring, "the common effect of most "diseases to produce derivation of blood to their "seats." "There is," in various diseases which have been particularly enumerated by Dr. Parry, "agree-"ment in the simple circumstance of a preterna-

* Exp. p. 161. † P. 162.

"tural quantity of blood."* "Hence the infre-
"quency of disease, without a consequent preter-
"natural fulness of blood-vessels." "By a reference
"to examples, it must be admitted, that the preter-
"natural derivation of blood is a circumstance which
"almost invariably accompanies diseases, whose seats
"are submitted to our inspection."

It has already been shewn, that Dr. Parry not only did not assert this condition to be an invariable one of disease, but maintained exactly the opposite doctrine. He had no hesitation in admitting, that there are not only cases, in which there is no preter-natural derivation of blood; but that in some instances disease† is associated with a deficient energy of the function, to which its presence has been imputed; and that there are cases in which the vessels, so far from being distended by a preternatural amount of blood, contain less than their ordinary quantity. We may further add, with regard to Dr. Pring's agreement with the views of Dr. Parry, that of ten conclusions adopted by the former, seven are not in opposition to Dr. Parry's views, and the three last respect a theoretical question, with which he had no concern.

Being satisfied as to the frequent occurrence of this implied condition of the sanguiferous system, and the relation it holds as a cause of disease, it may be interesting to consider the process by which this morbid change is effected. Though, with Dr. Parry, the Author of the Exposition of the Principles

* Pp. 168, 169, 171. † Exp. p. 170, 171.

of Pathology is disposed to believe, that, in most local diseases, the action of the heart is quickened, and "thus the supply of renovated arterial blood is ren- "dered more rapid," and that in some cases the accelerated action of the heart is synchronous with, or even anterior to, the local disease; he is, with him, also of opinion, that the latter state most commonly precedes such general affection; and, that if the heart originates disease, it must be from the parts being predisposed, or under the influence of re-agency.

According to both these authors, "the local ful- "ness which occurs in most diseases,"* arises from vascular dilatation. In the opinion of Dr. Pring, this state is induced, neither as a consequence of obstruction, nor as a condition alternating with contraction, movements, which, in his view, have no regular place in the arterial functions; nor as the effect of an active power, which he also denies. The vessels are dilated from an augmented volume or velocity of blood, following a state assumed by the structure to which the artery is distributed.† This state is an increase of the derivative power or function of the secerning system, and of the seats of its distribution, from which cause arise a preternatural derivation of blood to the part, and the visible signs of determination.‡

According to this doctrine, the ordinary business of circulation is promoted by a derivative or attracting power in the capillaries, the final cause of which is the function of the capillary secerning system. For

* Exp. p. 144. † P. 147. ‡ P. 166.

the purpose of secretion, besides their own peculiar function, the capillaries have also an affinity with the materials upon which they are to operate, and which they derive from neighbouring parts, and from the system at large.

If there were space for such an investigation, it might not be altogether irrelevant to examine, in this place, how far this process of capillary secreting affinity can be consistent with, or auxiliary to, the general circulation. It might be matter of examination, whether the secreting capillaries admit the entrance or passage of the whole blood: whether, by their function, they do not arrest, rather than promote, its motion: whether the blood so changed, and so arrested, might not require an increased instead of a diminished impulse for its passage through other vessels. It might be inquired, how far this process of analysis, this unremitting exertion of the secerning function itself, can provide a vis à tergo, which will promote the transmission of fluids: how far the function of the lymphatic absorbents can have an analogous power to that supposed to belong to the arterial capillaries, or, under the increased difficulties and obstacles to circulation, exercise, as may be required, even a higher degree of derivative influence; in short, how the whole process of venous circulation could occur, in which both the impulse of the heart would be partially, and the capillary auxiliary affinity would, as it appears, be wholly, wanting for the progress of the blood. These and similar suggestions

arise, under the consideration of this subject, in connection with the process of healthy circulation.

In the healthy state, the secreting function, a natural excitement, is continually drawing or determining blood to particular seats, for the support of life. This is the natural and healthy predisposition. Morbid or increased determinations are supposed to be effects, arising from preternatural excitement. As the final cause of the secretions is the renovation of the continually expending principle of life, and as preternatural excitement, on this hypothesis, occasions an increased local consumption of life, an increased supply is required of the material from which life is renewed.* Hence arises a derivation of blood in preternatural quantity. "It is," says Dr. Pring, "the common effect of irritation or excitement to "increase the function of parts. If the life of a "seat be under preternatural excitement, its con- "sumption is more rapid than usual: its assimilation "must be proportionally rapid, and the supply of "the material from whence it is derived, blood, must "be proportionally increased." Since, then, the circumstance of a locally† increased amount of blood is only a secondary occurrence, it appears, however impossible it may be to suppose such a circumstance, that this external excitement must exist in every case of determination; and as the effect of vascular excitement should be contraction and not dilatation, that no local effect is produced on the vessels themselves by this cause of change. The first previous local

* P. 179. † P. 167.

relation is between the life of the part, and the supposed excitement which is to consume it. Under the exhaustion of vitality thus induced, other vital powers receive an increase. An increase takes place of the peculiar property of secretion belonging to one part of the capillary system, which is immediately followed by the increase also of another peculiar property of the same, an affinity for blood. The process then, according to Dr. Pring, will be excitement, consumption of life, increased function of secretion, increased affinity for, and derivation of, blood. In nicely stating the relation of cause and effect, and arguing against the discovery of original causes, as the question has been argued in regard to determination of blood, it may be said that the derivative capillary attraction itself is no cause of the phenomena which follow, since the increased consumption of life is the pre-existing state of disease, which occasions the subsequent affection; and that here, as in Dr. Parry's fact of the dilatation of the vessels, we are still only engaged among the links of that chain, the beginning and end of which are still invisible.

But there appear to be many objections to the doctrine here laid down, and which, in very precise minds, might, perhaps, outweigh the force of argument by which it is supported. In the first place, it does not comprehend or explain all cases of determination, since there are many instances in which no evidence of local excitement can be adduced, whatever may be the immediate cause predisposing to the increased

afflux of blood. In most cases of predisposition, for example, a cause acting on another part shall induce a specific determination. The influence of moisture applied to the feet shall, in one case, induce sore-throat; in another, gouty or rheumatic swelling of a remote joint. Where, in these instances, are the local excitements necessary for the remaining effects, included under Dr. Pring's arrangement, unless we reason in a circle, and suppose the derivation of blood cause and effect?

In the second place, there is no attempt at evidence in favour of the position that life is locally consumed, even supposing such local excitement to have place; and it is not easy to reconcile the apparent inconsistency, that life is at the same moment, in the same place, and by the same cause, consumed or diminished, and preternaturally increased, as is supposed to be the case with the excited functions. If, indeed, conjecture may be permitted in so forbidden a subject, an increase of life of the part under excitement seems more probably to be coexistent with, or previous to, the derivation of blood. The derivation, then, could not be adduced in proof of the waste of life; and this effect is not required to support an increase of life.

In the third place, the increased function of secretion is also an hypothetical case, since, in no instance, is there proof of an increased separation of nutrient fluids, whatever increase there may be of the excretions, which themselves are also wanting in many cases of determination.

But, it is, fourthly, contended, under an implied admission of all the preceding data, that the supply of the material of life, drawn to a part under excitement, replaces, by assimilation, the loss of life which has been sustained. In inflammation, the most simple and palpable case of blood determined to a part by excitement, is, then, the matter separated under the increased function of secretion from the different surfaces which undergo this affection, for such support of life? or, is a distinct nutrient property of the blood assimilated in the injured texture, in a proportion with such excessive effusion; which effusion always coexists with the matter of nutrition? or what relation do the two processes bear to each other? Where is the evidence of a nutrient process in this instance of disease? It will be somewhat irrelevant to insist upon an abstract or spiritual nutrition, unaccompanied by the actual and visible signs of such a supposed change.

It does not appear that an increased function of secretion, the supposed immediate cause of the increased afflux of blood, can have for its final object the support of diminished life in a part, since, in the first place, even according to Dr. Pring, the afflux follows an increased condition of life; and in the second place, there is no evidence, that, under the continuance of such a state, nutrition goes on at all, even in cases where the excretions are not increased, and no additional waste can be supposed to arise from such expenditure. It may be repeated, that of the consumption and consequent diminution of

life, no evidence is supplied by this author, while he wishes very strongly to shew that the vital powers and functions, as well derivative as nutrient, are highly increased in the affected seat. It is scarcely necessary to call to remembrance, that in inflammatory determinations of blood, the most perfect instances of such affections, the body wastes in consequence of the want of nutrition. The blood itself, the material of nourishment and assimilation, appears, in these cases, to be changed in its quality, and is probably unfit for such office of nutrition, whether by retaining the principle of growth, or by holding it in a new and altered relation. It will also be admitted, that, whether this change in the blood occur at an early or late period of such affections, the afflux continuing, there is no ground afforded us by the hypothesis, for presuming the existence and application of any new cause. Under all these states, the chyle is perhaps imperfectly assimilated with the blood, and therefore whatever afflux may take place of the gross material, that principle, on which the renovation of consumed life depends, does not exist in a state fit for the presumed purposes of nutrition. It may be further asked, what connection appears between this afflux, and the supposed demand for nutrition, in cases of simple determination, in which there is no evidence of secretion or excretion, and in which the subsequent processes exhibit an effort to remove, instead of attracting, the preternatural abundance.

Having, however, thus far assumed the unqualified doctrine of nutrition, as the final cause of the in-

creased function of the capillaries, which, to this end, derive blood to their seats, Dr. Pring was aware of the objection which might appear to arise from the circumstance so generally known, and applicable to so large a class of diseases described by Dr. Parry, that determinations of blood occur without effusions; an objection, which, indeed, although the premises were correct, need have no weight, unless it were also proved that nutrition and excretion had a necessary connection with, and dependence upon, one another, or that the effusions were, in some respect, the evidence of nutrition. In order, however, to account for the supposed difficulty, and reconcile what might be charged upon his doctrine as an inconsistency, he abandons, in a great measure, his former ground, and supposes that there are distinct orders of capillaries, which may be similarly, and yet differently, affected. There are, in his opinion, capillaries of nutrition, which have a demand for nutrient fluids; and capillaries, opening on the surface, which also attract or derive blood, and which alone pour out the sensible secretions, in cases of determination.* Either the nutrient secreting capillaries, or the excreting secreting capillaries, may be the seat of irritation, and with some seeming opposition to the hypothesis lately stated, may derive blood to themselves. Whether surfaces shall, or shall not effuse their fluids, will depend, in this view of the case, on the balance of excitement, whether in favour of the vessels which excrete, or of those which "feed

* Expos. p. 173.

"the diseased condition." Now, if, as this author takes pains to shew, some properties of seats may be engaged without the implication of all the other properties, and if the nutrient can be engaged without the excreting function; and if, *vice versa*, it be admitted, that blood may be exclusively derived to the latter, for the purposes of its peculiar function, there seems no reason, why this should not frequently occur, and even why, in any case, that necessity should exist, of which there is never any evidence, and which is sometimes acknowledged to be absent. It does not appear, why, as in some cases, vessels not destined "to feed" may be excited, and there is no sensible evidence of nutrition in any case, it might not be philosophical to get rid of those nutrient derivative vessels altogether, and seek for a more palpable and consistent final cause of the effects which we witness in determinations of blood. In the instances adduced, there are, as we have seen, excitement, derivation, and separation, without the supposed final cause, nutrition, in vessels, having no direct relation with such an imaginary purpose. The vessels of excretion are sometimes the seat of irritation or preternatural excitement, and diseased states are thus accompanied by increased secretions.* But it is said to be only in the progress of affection, that such excitement and increase of function takes place; and in order to explain why the superficial secretions commence only as the pulse abates, it is imagined, that "the excess of excitement preponderates in one

* P. 175.

"order of vessels, and that the function of another "order of vessels comes to be exerted, when this "excess is moderated, and the energy or excitement "of the two systems rendered more equal."* However certain the fact may be, this account does not appear sufficiently intelligible, or very explanatory. The case which this author assumes is, that "if the "system of distribution of one considerable artery be "preternaturally excited, it will derive blood to the "seat in preternatural quantity;" that "the system "of contiguous vessels will be deprived of their "blood by such local excitement and preternatural "derivation;" and that these varieties in the acquisition of fluids are no less exemplified on that large scale, when the state of the circulation is compared in two limbs, than on the more minute one, when in the inflammation of an excreting organ as the liver, the system subservient to nutrition robs that which is subservient to excretion.† But, in the case before us, one set is not robbed, and the other does not act in excess. There is, in fact, at first, no evidence of either set acting at all, or, at most, of that set being preternaturally deprived of its blood, which, afterwards, profusely pours out its secreted fluids. But, giving the greatest latitude to the supposed case, let us examine the process which is described as an equalization of energies. The same exciting cause is applied to two sets of vessels intimately connected. One only acts upon such excitement, derives blood, and being satisfied, its function termi-

* P. 183. † Pp. 179, 180.

nates. We pass over the inquiry, how it happened, that this set alone was excited. The preternatural excitement continuing, the next set is supposed to take up the action, and attract blood, by which process a preternatural effusion is induced. Why, again, did this set now take up the action, or what is the order of its dependence? The original excitement may, however, not continue, and yet there having been no affection from previous irritation, whence do the supposed secondary effects in the excretory system follow? It is not probable that the influence on this set is derived from the nutrient vessels, as a diminished or abated energy would not easily constitute such a source of preternatural excitement. But, to produce the effect of effusion, the excitement must be excessive. In this process, then, which is not merely an exertion of the function of the excretory vessels,* but an excessive exertion, there is nothing to explain the occurrence of successive affection, or any thing like what can be termed, in an active sense, an equalization of energies. The latter set does not seem to take up the superabundance of the former, till the balance is established between the two. On the contrary other causes are in operation to produce the separate effects, which seem to have no sort of connection with, or dependence upon, one another. The nutrient secreting vessels expend their energies in nutrition, and the source of the excitement of the excretory vessels seems to be unconnected with any function of that former set. The explanation of this case,

* P. 183.

founded on the supposed equalization of the energies of the two sets of vessels, does not, indeed, seem satisfactory or consistent, and we must elsewhere seek a more probable solution of the problem.

But, in addition to those two already mentioned, there is, according to Dr. Pring, a third derivation of blood by the system of capillaries communicating with the arteries.* Now it was understood that the final cause of all determination was the supply of the nutrient material destined for the support of the life of parts, or in the author's own words, that "the "final purpose of derivation was nutrition."† Here, however, are two cases of afflux, in which such local support is not directly or indirectly indicated. Although, therefore, it may be safely granted, that the power of derivation, if it exist, is placed somewhere in the extremities of arteries, the conclusion does not seem inevitable, that, "under the state of preterna- "tural excitement, and consumption of the life of "minute spheres, a corresponding increase takes "place in the power which separates fluids from the "blood, which latter is derived to parts in this con- "dition in preternatural quantity, by an increase of "the power which is habitually excited."‡ There are, as it appears, numerous cases of determination of blood, unconnected with such direct purpose; others, in which, whether for the purpose of nutrition or excretion, no such sensible separation of fluids takes place, and "we have," as Dr. Pring justly observes, "no right to judge of what we do not see."‖

* P 175. † P. 176. ‡ P 177. ‖ P. 157.

Though, under his present illustration, it does not seem easy to admit the hypothesis of this author, as to the final cause of determination of blood, it appears, that he is himself not unwilling to admit other causes besides these, which have their locality, if the expression may be permitted, in the excited secreting and excretory functions. Notice has, already, been taken of the assumed causes of visceral congestion connected with an accelerated action of the heart, and disproportionate venous circulation.* Other final causes of the separation of fluids from the blood, in local determinations, are also admitted, when he says, "we know that secretion is an ending of in-"flammation, and that it is frequently the spontaneous "mode of relief to the vessels of an inflamed seat.†"

The immediate conection between the distribution of blood, in health and disease, and the "powers of "life," may be readily admitted, provided we are neither called upon to discuss, nor to explain, the principle which the admission seems to involve. We might, indeed, be disposed to examine the accuracy of the position, which ascribes to diminished vitality, and the consequent diminished derivation of blood, the reduced pulse, which is mentioned in certain cases described by Dr. Parry. For, though it will not be denied, that, when the powers of life are reduced, the pulse will be wanting or modified, it is not evident, that the vitality, and its dependent derivation of blood, is primarily diminished in the specified instances.‡

* P. 239. † P. 218. ‡ Pp. 178, 179.

In the case of loss of pulse, the coldness of the limb which accompanies it, and which, besides the reduced pulse, is the only symptom of diminished local vitality, may have been the effect of one, as well as of the other, assumed cause. It may have arisen from a diminished amount of circulating fluid, whether from a low state of life, or from an increased exertion of tonic power. The diminished pulse would, then, be the only evidence of reduced vitality, and this condition is otherwise explained by Dr. Parry. Though, however, according to this latter author, the approach of death produces a great degree of contraction in arteries, it appears, from Dr. Pring's examination of his experiments, that there is a single exception to this rule, and that, after a sixth bleeding, the carotid of a sheep, which had regularly diminished from $\frac{222}{400}$ to $\frac{127}{400}$, rose to $\frac{145}{400}$ before death. Whether this single exception should be deemed sufficient to vitiate the general conclusion, may be doubted. It will, perhaps not appear prudent to admit the hypothetical objection, that a local increase of vital power cannot have place under a generally diminished vital energy, in opposition to a fact so frequently true, and a cause so palpably analogous to that which produces similarity of effect in other processes of experiment. It is, in fact, to admit two opposite causes for the same effect, increased and diminished vitality, unless it be owing also to the latter cause, that immediately on the exposure of a carotid, it shrunk through the whole exposed space from $\frac{210}{400}$ to $\frac{188}{400}$ of an inch.

In the case adduced by Dr. Pring, in proof of diminished vitality, and consequently diminished derivation of blood, the opposite proof might, perhaps, be drawn from his view of it; since, according to him, a dilatation of the artery, consequent, perhaps, on an increased derivation of blood, took place before and after death. If he question the fact of contraction, and suppose an increase, instead of a reduction, of diameter, it should, consistently with his former views, arise from an increased volume, the consequence of certain increased vital functions. There is, however, in this case, neither excitement, nor cause for derivation. If he admit the fact of arterial contraction, is it explanatory, under such views, to ascribe it to a diminished vitality, which, if even for the purposes of support, should derive a larger quantity of blood. It does not seem very easy, or possible, to draw the distinction between a supposed local consumption of life from excitement, and its diminution, in consequence of any remote causes or affections, when the symptoms in each state are precisely alike.

But is there any thing particularly incongruous in the supposition, that local vital actions may have an increase of energy under the deficiency of general powers? or, might we not rather imagine, that such a local demand might be made for the security of the whole system, under an approach to general dissolution, and that those powers may be called into action, by which the important viscera may be supported, and supplied with the stimulating material of life, even at the temporary expense of the extreme parts?

Is not Dr. Pring's own hypothesis, in some measure, illustrative of this general fact? Does he not argue, that, under a diminution of local life, there is, in fact, a great increase of the vital powers of seats? Is not the demand, which he supposes, for blood, a consequence of diminished vitality, and is not the process by which it is derived, one of locally increased vitality, for the very purpose of replacing a lessened amount of life? But not to dwell on this apparent inconsistency, is it not true that, under similar circumstances, the action of the heart, and of the lungs, is known to increase, in the attempt to maintain the balance of the circulating and respiratory systems? In the very instance quoted, as adverse to the fact of contraction before death, the pulse, at the fifth bleeding, rose from 100 to 116, and may, indeed, itself have given occasion to the increased arterial diameter, which from 127 immediately increased to 140. The respiration, at the same time, increased from 14 to 24, with quicker and harder expiration; a circumstance, which itself might also greatly interfere with the previous uniform contraction of the vessel. In this case, the increased effort of the heart continued to the end, the pulse having risen from 124, through 68, to 168. This presumed exertion of the tonic power itself appears, in some respects, analogous to the increased muscular action, which takes place under certain circumstances, inducing convulsions. "Among the automatic efforts "to restore circulation, we may," says Dr. Parry, "perhaps, class the convulsions which sometimes

"accompany recovery from syncope, and the last "moments of life in animals dying of hæmorrhage."* It may also, perhaps, be compared with that locally increased pulse which occurs in some cases, whether ending in death or recovery. "I have often," says Dr. Pring, "known the pulse rather full than other- "wise in one wrist, and to be perceived but faintly "in the other."†

It will, however, be remembered, in answer to the original argument, that there is equally an increased exertion of the tonic power of the vessel, whether that be the cause of the diminished fulness, or the effect only of derivation elsewhere.

It is very evident, from what has been said, that Dr. Parry, in many respects, differs materially from the able author upon whose doctrines I have taken the liberty to animadvert. He does not attempt to explain the process by which blood is accumulated in a part under excitement, or in the numerous cases in which no external local excitement is perceptible; neither does his doctrine necessarily imply an increased amount of this fluid. In all instances, he presumes upon a difference of general or local susceptibility, towards the several effects which are manifest, arising from certain previous tendencies or dispositions; and admits, that, "whether local morbid "determination occurs from causes acting primarily "on the part, or secondarily through the medium "of excessive general impetus, it is equally difficult "to explain the process by which it is effected."‡

* Elem. p. 337. † Exposition, p. 177. ‡ Elem. p. 51.

Whatever this process may be, the palpable symptom induced, under accumulation of blood, is, as before observed, a dilatation of the vessels.

It may be remarked, in this place, that by the doctrine of capillary attraction, as a consequence of excitement, the morbid predisposition, which Dr. Parry asserts to be a cause of local affection, is rendered unnecessary, and, indeed, is presumed not to exist; for the effect of the excitement, without which, according to Dr. Pring, the preternatural capillary attraction cannot have place, is only an increase of a natural process suddenly induced, by the increase of its causes, in a healthy texture. In excluding the blood itself as a cause of affection, we fail also to explain the cases in which predisposition alone, or a spontaneous capacity of dilatation, is assumed as a cause of the subsequent morbid phenomena, as may happen in all cases of specific local affection from general causes of excitement.

Having confined himself to the state of the vessels, as established by the evidence of the senses, Dr. Parry attempts an explanation of the final causes of determinations, deduced from the previous sensible conditions, and from certain subsequent changes no less palpably manifest. The final cause of determinations is, upon this principle, either evacuation, "a process expressly designed to mitigate or cure "the disease," and the consequent reduction of a plethora, which is positively or relatively excessive; or the restoration of the due balance of the circulation, previously determined in excess towards other

and more vital parts.* The effects are, hæmorrhage, effusion, or congestion; of which, the two first affections readily explain themselves, and the third, as inducing also the convulsive state, finds a beautiful illustration, and explanation, in the doctrines of this author. According to Dr. Parry, in parts predisposed for such a state, and where, as happens in numerous cases, there is an absence of external excitement, the derived blood itself acts as a local irritation, inducing the preternatural exertion of the local functions, and the consequent salutary results of hæmorrhage, effusion, or convulsion. Whether effusion or other effects shall follow, will depend on various circumstances connected with varieties of force, the nature of the texture, and with predisposition. The degree of morbid excitement is in a compound ratio of the predisposition, and the force of the exciting cause, whatever it may be.† So also hæmorrhage, like inflammation and dropsy, is not directly as the local disposition in the vessels, or as the degree of the momentum of the blood, but in a ratio compounded of the two.‡

That excess of blood is frequently productive of disease, in consequence of previous functional derangement or predisposition, is admitted by Dr. Pring;‖ and it is also admitted, that when it is not so productive of disease, in constitutions not locally predisposed, such evil is prevented by the limits which are placed to excess, in diminished appetite,

* Elem. pp. 63, 82, 122, 137, 345, 353. † P. 236.

‡ P. 131. ‖ Exposition, p. 195.

impaired digestion, the production of fat, and other provisions against repletion.* In all this, he agrees with the Author of the "Elements," and with others who have preceded him.

On the subject of the Cure of Diseases by Conversion, a term and discovery which Dr. Parry disclaims as his own, and a fact, for the knowledge of which he refers to all experienced men, Dr. Pring endeavours to shew, that, as determination of blood is the effect of local disease, and only a cause concurring to maintain it, so a substituted determination is only one circumstance where many are involved, and, therefore, is not itself the cause of the cessation of a disease.† There would have been as little difficulty with Dr. Parry in admitting this statement, as, that disease itself involved many other circumstances, besides mere amount of blood; and, however ingeniously Dr. Pring may explain the fact, asserted by the former author, that artificial determinations fail, in too many instances, to produce the benefit effected by natural substitutions; it is to be feared that we do not acquire any very precise knowledge, or obtain any useful practical information, when we are told that such artificial means fail, "unless they have the "same relation of substitution with the diseased state "as the natural."

In reviewing the observations of Dr. Pring in regard to the subject of determinations of blood, it appears that many of the important points for which he contends have never been disputed; that his

* P. 198 † P. 206.

experience frequently confirms the peculiar doctrines of Dr. Parry; and that, occasionally, his speculations, however interesting and profound, are not very intelligible, very tenable, or of great practical utility.

When he asserts that the state of disease is complex, involving a change of properties, "the nature "of which cannot be defined;"* that the local state of the vessels depends on a state of disease, "the "precise nature of which cannot be ascertained;"† and that, in the circle of relations formed by each several organ under disease, "it is difficult to decide "on which the phenomena displayed primarily de- "pend, or the share in these phenomena which might "be imputed to each,"‡ we are satisfied that even the high analytical powers which, in this acute writer, confessedly command our admiration, are as little competent to the investigation of primary causes as our own more limited faculties; and that, probably, there is more safety, and as much convertible value in the confined view which we are obliged to take, as in the abstract and remote speculations of this eminent and endowed metaphysician.

As the derivation of blood is a part of this obscure and unintelligible local disease, and as there is no sensible evidence of any change in the vessels themselves, and still less among the properties connected with life, in whatever structure we seek them, it may be more prudent and philosophical to remain satisfied, at present, with the doctrines of Dr. Parry; and admitting the local and palpable circumstances of the

* P. 200. † P. 192. ‡ P. 249.

circulation, whether increase or diminution of quantity or velocity, as fit and important subjects for our attention, and partly as the guides for our practice in the cases especially pointed out by him, to reject, as unsuited to our capacities, and inapplicable to practical purposes, the unknown and invisible conditions, which are dependent on causes inscrutable and too remote.

Having considered the pathology of Determination, Dr. Pring proceeds to examine the treatment said to be suggested by it. Here, again, the remark cannot be forborne, that his observations do not apply to the opinions, or practice, of Dr. Parry. "The principle," he observes, "that all diseases are produced "by a local excess of blood, suggests a very easy "plan of therapeutics. As excess of blood is the "cause of disease, there is nothing to be done but to "diminish it, and to persevere in this plan as long as "there is any disease."* Dr. Pring, on the contrary, gives it as his opinion, that bleeding is by no means a *universal* remedy;† that the abstraction of blood is not a measure to be *wholly* depended upon; that *excessive* blood-letting is often attended by fatal consequences; and that *the mere* abstraction of blood is inadequate to the cure of many diseases.‡ In answer to such remarks and suggestions, I would ask, first, whether they have any reference to the expressed or involved opinions of Dr. Parry on the subject of blood-letting; and, secondly, whether in

* P. 203. † P. 203.

‡ Pp. 204, 205, 236.

his writings or practice there was ever exhibited a disinclination, on his part, to admit "the co-operation "of bleeding, blistering, purgative, nauseating, or "even emetic, medicines," in cases where such remedies are indicated or usually employed? With regard to a case of pneumonia, in which "the feet "and legs had become red and anasarcous, with a "pulse of 130 in a minute," we may safely leave to Dr. Pring and his patients all the benefits and satisfaction, which, according to the very extensive experience on which he so implicitly relies, must decidedly result from the co-operation of "a diet of "beefsteaks and raw onions, a quart of porter, and "two or three glasses of gin and water per diem, "with scarcely an aperient once in three days;"* and we may think it, in such a case, quite unnecessary to defend Dr. Parry against the imputation of distrust or ignorance. Neither can we feel any wish to interfere with the comfortable recommendation of "a full diet," consisting both of animal food and malt liquors, "to patients suffering under nervous "disorders of the head."† These and many similar examples of practice, which we readily admit to be in direct opposition to the knowledge and experience of Dr. Parry, we as readily leave to their own fate. Neither have we any concern with various practical remarks and illustrations on the employment of certain co-operating remedies, which have been employed by Dr. Pring in the cure of divers diseases, and which, however successful, do not seek the merit or

* P. 206. † P. 246.

reputation of novelty. To the experience which he admits to be the only safe guide for practice, we also leave the question, whether, in inflammatory diseases, blood-letting should be principally depended on, or be only an auxiliary to measures which are more powerful.* It is, however, satisfactory to find, that blood-letting is admitted to be a highly important measure; and that the objections are not to its judicious employment, but to an entire confidence in it.† As a remedy, in some forms of acute disease, its merits are said to be very obvious;‡ in others its efficacy is believed to be inferior to that of some other means; while, in some chronic diseases, of which determination of blood is even a conspicuous symptom, its propriety, and the extent to which it should be carried, are, perhaps, of more difficult discrimination. After all, we learn, from the same authority, that the test of the curative effect of blood-letting is experience.§

That blood-letting was not considered an adequate remedy for all diseases, even in which determination of blood was a conspicuous sign, unless it has a curative relation with the state of disease upon which the symptom depends,‖ is evident from all Dr. Parry's doctrines and practice. It is, indeed, greatly to be lamented, that we have not even the outline of his therapeutical experience, and that the public have received no sufficient information as to his particular treatment of diseases. This regret is increased by

* P. 215. † P. 217. ‡ P. 226.
§ P. 228. ‖ P. 253.

the evident ignorance of his views, manifested in the remarks of those who undertake to review and criticise his supposed principles. We may, however, now refer, with confidence, to his numerous cases and correspondence, a detailed publication of which has been rendered necessary by the absence of any more compressed histories, and by the justification required, as in the present instance, against observations and statements, as inconsistent with his real views and opinions, as injurious to the cause which he maintained. So far from recommending indiscriminate blood-letting, wherever we have an opportunity of knowing his opinions, Dr. Parry has taken pains to guard against such an abuse. In cases of excessive determination to the head, he speaks of blood-letting as beneficial "under certain modifica-"tions."* He accurately discriminates affections of the head, which, though apparently similar, do not equally warrant its employment; and points out the distinction, which, in seemingly analogous states, would establish the danger to be apprehended from its use.† He has hinted at the ill effects which result from excessive blood-letting, and, in various parts of his published works, anticipated the suggestions, and superseded the necessity for the remarks, of Dr. Pring. How little he trusted to blood-letting as a cure of diseases, even in which determination is a conspicuous symptom, is sufficiently manifest in his writings. It may, indeed, appear superfluous, to those who were acquainted with his

* Elements, p. 246. † P. 247.

practice, to particularize his various employment of other remedies, to exhibit the dependence which, in many disorders, he placed on "cool air, temperate "living, adequate rest, and every thing which quiets "the action of the heart;"* how much, in several modifications of nervous disorders, he relied on "voluntary exercise of the body, by means of its "own muscles, as the best preventive of all the "diseases depending on excessive sanguineous deter- "mination, and, when properly adjusted, as an essen- "tial part of the cure of the far greater number of "them;"† how little disposed he was to direct his remedies to the cure of local affections, by which, in his opinion, we are, strictly speaking, prescribing only for symptoms ;‡ and how averse to the "admi- "nistration of certain internal remedies, the tendency "of which is not to cure the constitution, and to "remove the necessity of the local disease, but "merely to check the present salutary action of the "system, and thus to cause only a temporary and "delusive suspension of present suffering."§ Much as we must regret the want of a more exact and particular history of his practice, we may nevertheless consult with advantage a statement of the remedial measures which he adopted in certain marked cases of determination of blood, which was published in the Memoirs of the Medical Society of London in the year 1788. As a reply to the mistaken observations which have been made, in reference to his doctrines, by many authors who have not been at

* P. 242. † P. 335. ‡ P. 360. § P. 364.

the pains to study them, a full insertion may be permitted, in this place, of this original abstract of his opinions on this important subject.

"The view," he observes, "which I have given "of excessive determinations of blood to different "parts, points out the mode of practice to be adopted "in such cases. It consists in the use,

"1*st*, Of those means which diminish the undue "action of the heart and arteries in general;

"2*dly*, Those which repress the excessive action "of the arteries of the affected part; and

"3*dly*, Those which excite or restore determina- "tions to other parts of the arterial system.

"Under the first head we may rank the avoiding "of all general stimuli, whether external or internal: "a low diet; general blood letting; many evacuants "which will be mentioned under the third head; "saline refrigerants; and a set of remedies, many of "which will evidently, and all probably, have an "immediate power, in the greater number of cases, "of diminishing arterial irritability, such as medicines "producing nausea, opium, henbane, aconite, hem- "lock, tobacco, foxglove, lead, and perhaps arsenic, "zinc, and copper. Of the second class, are topical "bleeding, and other evacuants, and cold. Of the "third, are rubefacients, friction, vesicatories, elec- "tricity, heat, cold, purgatives, diuretics, diaphore- "tics, sialogogues, and sometimes, though rarely, "and with the utmost caution, stimuli taken into the "stomach. This system of practice I have tried,

"and with a success much greater than that which I "had long experienced from the remedies commonly "employed. As my time has chanced to permit, I "have collected numerous facts, which not only "support the general principles which I have formed, "but point out the particular modifications of those "principles. These observations will, I think, ex- "plain the important influence which the stomach "has on the arterial system; and how a great variety "of causes, some of which are in their own nature "opposite, come through different roads to the same "point, and produce the same ultimate effects in the "animal economy." "In the mean time, I flatter "myself, that those who by anticipation would treat "my system as hypothetical, will suspend their deci- "sion, when they are informed, that it is nothing "more than an attempt to describe the invariable "order of phenomena."

Of the extension and various modifications of these views, which were admitted by Dr. Parry in his subsequent practice of nearly thirty years, it is not necessary here to speak. It is sufficient to say, that, in this period, he augmented, rather than abridged, his catalogue of remedies, and that many of his usual means are not included in the above summary of his practice.

As it appears that the doctrine of Determination of Blood was never considered otherwise than "defective," and was never proposed as explanatory of all the facts of disease, while the state itself was

especially declared to be only a concurrent cause,* even under its most important and common modifications; so is the assumption equally inconsistent with a knowledge of these doctrines, that the therapeutics suggested by them consisted chiefly in bloodletting, and that "extreme depletion" was the whole and sole means of cure. There has, probably, appeared sufficient reason for believing, that the charge of being defective in principle, and inadequate in practice, is wholly unwarranted and misapplied, as directed against the opinions of the Author of the Elements of Pathology and Therapeutics.

* P. 236.

CHAPTER IV.

NERVOUS DISEASES.

In the preceding pages it has been my endeavour to shew that Dr. Parry was not disposed to consider determination of blood, or derangement of the sanguiferous system, as the sole and universal causes of disease. It is now necessary to extend the inquiry, and to demonstrate, that, in his consideration of morbid conditions, and from a supposed preference to this part of the animal economy, he did not, as has been most incautiously asserted, overlook other circumstances of the constitution, equally important, if not equally conspicuous. It has, indeed, with some apparent plausibility, but with little regard to their intrinsic merits and tendency, been objected to his doctrines, that they exhibit "a total disregard "of the nervous system;" and this objection has, with many, been urged as an impediment to the adoption of opinions and principles, otherwise admitted to be highly judicious and important. The

chief ground of this objection appears to be derived from a passage in the Preface to his Elements of Pathology. It may, however, be safely asserted, that, if the expressions there employed have been condemned, they have, in at least an equal degree, been misunderstood. Before an attempt is made to define the real influence of the nervous system in the production of disease, and to examine the importance of the doctrines which are at present entertained on this subject, it will be proper to ascertain with what justice this charge is advanced, whether as referrible to the passage in question, or to the general tenor of Dr. Parry's opinions.

In the place to which allusion has been made, this author thinks it necessary to make the introductory observation, that his principles are altogether derived from phenomena or facts cognizable by the senses, and arranged in their natural order. "Conceiving "this to be the only mode of investigating and "arriving at the truth, he utterly and for ever dis- "claims all reliance on the neurological systems of "pathology hitherto extant. He considers them as "founded on principles, which are either visionary or "inapplicable, and which lead to practice, tending "equally to debase the moral character of mankind, "to produce or perpetuate disease, and to discredit "the medical profession."*

Can this passage, by any fair construction, be considered a disclaimer of the intimate connection of the nervous system with diseased conditions? By

* Elements of Pathology and Therapeutics, Preface, p. v.

any forced interpretation, can it be made to imply, that, in pathology, its Author sanctioned a total neglect of this system, and imagined that, whether primarily or secondarily affected, it did not bear an important part in the complex relation constituting disease? Does it include the slightest hint, that the sanguiferous and nervous systems are not mutually dependent on each other; that we must not attend to both, "if we wish to practise with success and "discrimination?" Or, is it not rather a denunciation of the false theories, which, up to his time, had been admitted, and of the illegitimate conclusions and injurious treatment which had resulted from their adoption? An exact idea of the nature of philosophical inquiry, and a marked success which had attended his own practice, led him, it is true, to renounce for ever the hypotheses and the fashions of the day. But, in stating the benefits of observation and experience, however he may have felt the improbable circumstance, that in the nervous system should ever be found a very safe or palpable guide for medical treatment, has he any where assigned to it even a subordinate influence in the causation of disease? Has he any where denied, that "the nascent "movements of disease may not be found in the "sentient system," or that the condition "even of "inflammation" may not have its primitive morbid source in an impression on this part of the constitution? It is scarcely necessary to prove at length, that he did not consider the sanguiferous system the sole receiver of impressions; and that he did not,

therefore, place the original and universal excitant in this part of the animal economy.

From the general doctrines of this Author we learn, that "while in proportion to the rank which "created beings were destined to hold, and the "additional functions suited to that gradation, more "parts were added, which, for the purposes of minutely "varied and reciprocal influence, require numerous "communications with each other and the whole,"* the functions of the nervous system met an ample and due attention, and, in their connections with disease, received from his inquiries an extended and unexpected importance. With those who preceded him, he considered the brain to be the material organ of all the mental faculties,† the ultimate organ of sensibility, and the chief medium of the greater number of animal motions; and the nerves to be the media of sensation. While he believed that sensations and ideas stimulated the brain and mind to their affections and functions, he saw that, in their turn, these act on the brain and nervous system, and through them on a part or on the whole of the blood-vessels; that the mind, through the brain, acts on the sanguiferous system, and that this re-acts on the brain; that there are various causes of excitement to the brain and nervous system;‡ and that increased impetus of blood was one concurrent or co-operating cause in the production of many nervous affections, which again in their turn influence other parts of the constitution. That an anterior origin of disease is

* Elm. p. 185. † Pp. 200, 201, 226, 234. ‡ Pp. 235, 6, 7.

in the brain or nervous system, he illustrates by his doctrine of nervous disorders, in which the brain communicates an impression to a morbidly irritable heart, which determines the blood with excessive violence to the brain, and causes it to re-act through the nerves on various other parts, and thus to produce the phenomena of the diseases attributed to the nervous constitution. In his doctrine of spasm and insanity, he shews the proximate cause of diseases to be in the brain;* and places the origin of other nervous affections in the spinal marrow and other parts of the system,† as a consequence of irritation of medullary substance, whether of the brain, spinal marrow, or nerves. He endeavours to prove that the common nervous theories furnish no determinate ideas; and inquires, whether, in certain maladies, there may not be morbid states cognizable by our senses, and more capable of indicating rational and efficacious means of relief.‡

With regard to the opinion strongly expressed by Dr. Parry, as to the insufficiency of the neurological systems then extant, does it appear, on a candid examination of the sentiments of those authors who have most cautiously inquired into the subject of nervous diseases, that they are not really obliged to adopt a nearly similar conclusion? Setting aside all claim to a priority of discovery, which might be established upon sufficient grounds, does it appear that, at the moment of publishing his doctrines, there was in fact amongst authors any great diversity of

* Pp. 267, 8; 276. † P. 282. ‡ P. 293.

sentiment, or that even up to the present period the opinions of physiological or pathological inquirers, as to the obscurity, and therefore the insufficiency, of the doctrines connected with these intricate subjects, differ materially from those expressed by Dr. Parry himself. We have the recorded opinion of an excellent physiologist, Mr. Carlisle, who, about that period, remarked, "from a continued series of faithful "descriptions of the nervous structures, and their "relations in animal fabrics, extending from the most "abstract example of a distinct animal up to man, "some important results may be gained; but neither "my experience as a medical observer, nor my re-"marks as a naturalist, have hitherto produced any "satisfactory explanation of the proximate causes of "nervous diseases, of the rational means for treating "them, or of the intrinsic nature of that quality or "power which belongs peculiarly to the brain and "nerves of animals."

Mr. Pring, who, in 1815, published the best work which had appeared on the subject of the nervous system, however his opinions may have been corrected by subsequent investigations, begins his view of the relations of the nervous system in this manner: "There is, perhaps, no subject in medicine, concern-"ing which so much has been said, so much desired, "and so little understood, as that of the diseases "of the nervous system. We need not inquire "with much diligence for the reasons of this dearth "of knowledge: it arises from the difficulty of un-"derstanding causes, where the senses can inform us

"only of effects. The same difficulty still continues; "and it will both outlive medicine, and every other "monument of the art of man."

By a reference to various other distinguished authors, it would be possible to prove, that, according to their opinion, the pathology and physiology of the nervous system consisted at that time of a mass of heterogeneous and discordant matter; that hypothesis and unwarrantable assumption were the basis of all that was done or attempted; that the presumed experience of many profound philosophers had, in fact, no foundation in real knowledge; that "proofs were "deficient and fallacious, explanations unsatisfactory, "and the facts and experiments inexplicable and "inconsistent." Shall we not, then, rather justify, than blame, Dr. Parry for a fair and decided renunciation of the "neurological systems hitherto extant?" Shall we not rather extol, than censure, his endeavours to escape from obscure and mysterious indications, and to discover in the maladies of the human frame, and particularly in those designated nervous, "certain morbid states cognizable by our senses, and "more capable of indicating rational and efficacious "means of relief?"*

That this author was not, without reason, averse to the false theory which dictated the usual treatment of nervous disorders,† is sufficiently proved by the present almost entire disuse of whole classes of medicines and remedies, upon which the greatest reliance, derived rather from a nominal than real efficacy, had

* Elem. p. 293. † P. 150.

hitherto been placed, and by the adoption of many very opposite modes of treatment, which, as can be amply demonstrated, were first recommended by himself.

But, if the opinion, that Dr. Parry unjustly disclaimed the neurological systems of the day, cannot be supported; is it evident that he equally renounced all possible future advancement, and the knowledge which might subsequently be gained on these intractable subjects? Nothing can be made less evident from the passage in question, or from the general tenor of his writings, or of his inquiries.

It may, however, be asked, with regard to the primary influence of the nervous system, as an origin of diseases, what is even now done, by which we are much in advance of Dr. Parry's views? Where are the incontrovertible facts, derived from precise experiments, by which such a condition can even at the present moment be demonstrated, or on which a successful treatment of the first stage of morbid affection can be established. If we cannot so demonstrate the primary source of disease in affection of the nervous system, have we, at this later period, in its secondary affections, a more important and more tangible connection, than that which is discovered in the influence of the sanguiferous system? Have later writers, with whatever caution and zeal they have prosecuted their inquiries, advanced much in this dark path? Is the cloud removed, which veiled with palpable obscurity the phenomena of nervous affections? Has precise and applicable knowledge taken place of those

indistinct speculations, which, while they seemed to explain, confounded the understanding, and misguided the practice. In the ingenious researches and interesting deductions of the present day, do we find that their authors have in illustration so far succeeded, as yet to claim for their neurological systems an entire confidence and approval? Or, in successful treatment of the various maladies in which the nervous constitution is chiefly implicated, have they attained, what should be the result of accurate conception and defined principle, an infallible guide for practice? When they employ the terms nervous sympathy, nervous irritation, nervous debility, nervous energy, nervous medicines, &c., are the phenomena to which they relate of so easy access, and so readily explained, as to be liable to no possible cavil or misinterpretation. Do we not, on the contrary, find it admitted by the most intelligent and candid authors, that "an impenetrable obscurity"* still pervades this subject; that "the nature of the changes in the nerves "is not demonstrated;"† that if we proceed a step beyond the gross changes in the animal economy, we find ourselves "much in the dark;"‡ that the nervous "system is, for the most part, hidden from our "view;"§ that "the mobility of this system is "hypothetical;"‖ that "the parts are so constructed "as to escape observation;"¶ with numerous confes-

* Dr. Cooke, on Nervous Diseases, p. 152.

† London Med. Phys. Journ. vol. xlviii. p. 234, 5.

‡ London Med. Rep. vol. xviii. p. 392.

§ Hastings on Infl. p. 108. ‖ Cramp's Inq. into Opium, p. 166.

¶ Swan's Obs. on the Nervous System, p. 80.

sions of a similar nature. It is, indeed, the opinion of the author first quoted, who has, with much industry, collected all that is known, "that, notwithstanding the laborious investigations of the subject "by philosophers in various ages of the world, the "physiology of the nervous system, as to many particulars, remains involved in impenetrable obscurity."*

That primitive impressions, and, therefore, that all diseases arose from the nervous system, as an early link in the chain of causation, appears not to have been denied by Dr. Parry. How, then, is a total disregard of the nervous system deducible from his doctrine, that after an impression is made, after the operation of certain causes has become manifest, the phenomena in most instances arise from a modification of sanguineous impulse; and that, both in theory and practice, this part of the series is the ostensible, and therefore the most important connection? It may, indeed, without any extravagance, be asserted, that, notwithstanding the rank which is now generally assigned to the nervous system, these conclusions of Dr. Parry derive irresistible force from the experiments and observations of all those writers who have succeeded him; and that as, by the labours of later experimentalists, it is clearly evinced that the primary effects in the nervous system are neither visible nor manageable, we cannot, consistently with prudence or philosophical precision, found our neurological or therapeutical distinctions on a basis so uncertain, and so easily destructible. According

* Dr. Cooke, p. 152.

to Dr. Pring, the sensible causes of diseases are the proper subjects of treatment. If, with this author, it be readily admitted, that the nerves are a principal seat of the organic life, and the media of reciprocal influence exerted by the organic spirit in its different seats; is it not equally admitted, that the properties involved in disease are among the latent properties; that the precise nature of the change from health to disease can rarely be known, even imperfectly, because the agents are, for the most part, not objects of the senses; that the plan of therapeutics accomplishes its objects generally through the medium of relations of a complicated kind;* but, that there is a relation through the material connections of the spirit, of which the most important is that through the medium of the blood?†

While, however, it is nominally the fashion to run into an opposite extreme, and to exalt the nervous system as "all in all;" to dwell with emphasis on the inscrutable relation which this system bears to morbid effects, as a primary source, or as an instrument of disease; and while the most opposite meanings are still assigned to an unintelligible nervous phraseology; it is very evident, that those who examine the matter without prejudice, deduct very much from this imagined importance, and circumscribe within more narrow limits the peculiar merits and influence, even of the systems of neurology belonging to the present day.

* Page 495.

† View of the Relations of the Nervous System, p. 251.

What is there, in fact, in the doctrines recently developed, which supersedes or contradicts those which inculcate the extensive and important influence of the circulating system in its admitted connection with the various maladies of the human frame? Is not this system the most obvious and secure guide for our practice? Are not the rules of our experience, derived from a consideration of its relations, of far more extensive and of more certain application, than those which are founded on an imaginary excess, deficiency, or modification of nervous influence? Are the organic changes in those finer parts of our mechanism so distinctly cognizable, as to offer evidence of their existence during life, or alone to indicate, after death, the precise nature of the fatal malady? Are the changes in the brain itself, for instance, so perceptible, as to point out a determinate relation with any pathological or therapeutical views? Will it be contended that the functional or even organic changes of the nervous structure are, in any respect, matters of sensible evidence?

In taking a candid review of all that has been done by modern neurologists, it will appear either that they still justify, by their admissions, the supposed exclusive doctrine of Dr. Parry; or, when they are practically and philosophically consistent, that they generally arrive at the same conclusions with himself. The nervous changes and phenomena are still invisible and mysterious agencies; and the circulating system is adopted as the great intermediate link in "far the "greater number of maladies." As with Hunter,

the blood was the first moving power, and with Le Gallois and other physiologists, life is the result of the impression of arterial blood, the most considerable and constant of the exciting powers, while it is known that the most vivacious animals have the least nervous system; so is it also suspected, even by some writers of the present day, that the disorders of the nervous system may, in the majority of cases, be only secondary and sympathetic affections.*

When, indeed, mention is made, by authors who are most attached to the supposed results of this system, of stimulants, whose effects are comparatively more exerted on the nervous than on the sanguiferous system, or of affections, of headach for example, which is a mere temporary affection of the nervous system; it may be inquired, whether it is clear, from evidence that cannot be controverted, that the dependence of the phenomena is exclusively on this system? Is their nervous irritation wholly unconnected with vascular pressure or inflammatory action; or, with those who adopt the muscular irritability of arteries, is the effect of irritation necessarily to be sought in the nervous structure alone? Passing over the numerous assumptions in regard to nervous energy and nervous debility, and looking entirely at the practical side of the question, are the pathology and therapeutics so clearly derived from accurate physiological knowledge, as to justify us in underrating the extensive, applicable, and sufficiently certain means offered by the experience

* See Dr. Pritchard, and others.

of the pathologists who prefer a reliance on another part of the constitution; and who, though they admit, that "all the parts of a living body are con-"cerned in its diseases, and that it is only as we "advance in a knowledge of these various parts, "and their relations to each other, that the nature of "its deviations from a state of health can be under-"stood;"* build their curative plans on certain sensible organic changes, and the more evident facts of the circulating system.

In examining more minutely the question of our real advancement in a knowledge of this subject, since the period when Dr. Parry rejected the speculative notions of his time, it is, indeed, not very easy to discover the superiority demanded by those who maintain, according to the fashion of the present day, the paramount importance and connection of the nervous system. However much gratified the mind may be with the high analysis of Dr. Pring, or the admirable deductions of Dr. W. Philip, we still do not feel quite satisfied with the results of so much labour, and so much indisputable talent, as have been employed in the investigation of these abstruse subjects. From the observations of the distinguished author first named, we may, indeed, learn not only how limited is our acquaintance with these invisible properties, but, at the same time, clearly estimate the real value of that knowledge which is derived from an attention to the facts of the sanguiferous system. When we are instructed that life is

* Dr. W. Philip, on Indigestion, p. 123.

not exclusively allied with the nervous system, nor conferred by it alone; that the organic life is independent of the nerves, but that its energy is diminished by the privation of nervous influence, which has no agency above that of a stimulus; that where organs, besides their characteristic organic functions, have peculiar ones added, as in digestion &c., the properties of nerves are essential as an auxiliary to those functions;* that the function of nerves in health is, at most, only a related one with other properties, which, judging from their phenomena, are infinitely more numerous and important than those of the nerves; that as the nervous centres, like the heart, have only a common relation with parts, their influence is derived from local states of disease; that increase of nervous influence succeeds to a state of disease; that whatever share nervous influence may have in the symptoms, the state of this influence is preceded and governed by a specific condition of local disease;† that the examination of certain doctrines of pathology, connected with the nervous system, is made under the disadvantage of a doubtful and imperfect physiology, which at present belongs to these organs. When we learn, further, that the spiritual influence is primitive; that the stages of organization, with which they are allied, are only secondary; and that the nature of diseased change of life does not admit of being specified; we may, indeed, safely throw back upon the nervous theorists, those who most loudly exclaim against the

* Exposition, p. 332. † Pp. 342—3.

insufficiency of the principles adopted by Dr. Parry, that even they are still far behind a discovery of any of the real principles of morbid affection, and are still very remote from any acquaintance with the real phenomena of causation.

In the pathological lessons of these very acute writers, we have, indeed, the most decided evidence of the fallacies and imperfection connected with this vaunted doctrine of nervous causation. When, among the mysteries and hidden things of this system, it is admitted, that even "the structure of the medul-"lary substance is doubtful;" that, of four circumstances necessary to the health of the brain,—the presence of a perfect principle of life, the aggregation of its proper matter, the arrangement of this matter in consonance with laws, which, from their minuteness and obscurity, are not understood, and the presence of arterial blood,—the last alone is a tangible and evident fact of the constitution:* that "the "mode in which sensation is conveyed, is a fruit-"less inquiry;" that "of various modes of nervous "sympathy, it is doubtful which is the real one," and that on this subject "we may as well acknowledge "our ignorance by this word sympathy, as by any "other;" when, in relation to the ultimate pathology of certain nervous diseases, it is admitted, by the able inquirer already mentioned, that "we have no satis-"factory information, no direct principle for its cure, "no permanent mode of intercepting central commu-"nication:"† that, again, with regard to another,

* Pring's View, pp. 21, 23, 27, 175. † Pp. 109, 117, 150.

"its condition consists in a modification of the pro-
"perties of the nerve, that it is a peculiar condition,
"an effect, from a state of the brain giving rise to
"diffused spasm, in consequence of predisposition in
"a doubtful structure; that the precise condition is
"ascertained by no sensible testimonies, and that
"there is no mode of discriminating its extent; that
"the latent properties of nerves are the agents of
"sympathy, and that the history of predisposition
"to specific effects is not traceable;"* when we consider these, and an infinite number of other, admitted instances of entire ignorance on all the prominent pathological points of inquiry; it remains for the advocates of the nervous origin and connection to shew, what superior reliance should be placed by us on a supposed knowledge of such obscure and of such inexplicable indications.

But it will be said that other experimentalists have, perhaps, carried the practical examination of this subject beyond the able author just noticed. With every wish to do justice to the beautiful and perspicuous exposition of his views, which has been more recently published by Dr. Wilson Philip, as well in his Review of some of the general Principles of Physiology, as in his Treatise on Indigestion, an extended research may not, perhaps, satisfy us that any material advance has been made, even by this distinguished writer, in our knowledge of those points which more particularly interest the pathological inquirer. What are, in fact, the points, connected

* Pp. 239, 240.

with the diseases which are assumed to be essentially nervous, in contradistinction to effects in which the sanguiferous system is interested, which have by him been elucidated, or established? In physiology, we may refer to the subjects of voluntary and involuntary motion, and secretion; and in pathology and therapeutics, to indigestion and some of its consequences, pure nervous headach, pure nervous apoplexy, and permanent asthma.

The experiments of Dr. Wilson Philip lead to the inference, that, in the action of the voluntary muscles, the nervous influence is, under the direction of the will, their sole natural stimulus, but that they can be affected by other agents, chiefly of a mechanical kind, applied to the origin of their nerves alone, and may, by such irritation, be excited to most irregular movements; that, while the muscles of involuntary motion have no direct dependence upon the brain and spinal marrow, and in all their usual functions are excited each by its proper stimulus, they can, however, occasionally be excited by the nervous influence; that they are more affected by chemical than by mechanical agents, and by stimuli applied to every part of the brain and spinal marrow. Thus the heart is influenced by all agents applied to any considerable part of the brain or spinal marrow, while the muscles of voluntary motion are only influenced by the more powerful agents, applied to certain small parts of those centres. Spasm, or irregular action, will then arise from stimuli acting partially, or interruptedly, on that part of the brain and spinal marrow from

which certain nerves arise. The muscles of involuntary motion, however, as they require a diffused impression to be made on a large surface of the brain, cannot, it is imagined, be acted upon interruptedly, as the muscles of voluntary motion may be affected by successive stimuli, applied to individual and confined parts of that viscus.

Allowing these important inferences to be correctly drawn from accurate experiments, what purely nervous or exclusive agency do they, pathologically speaking, manifest? or, what want of connection with the influence of the sanguineous system in disease? Do not the relations both of voluntary and involuntary actions shew, on the contrary, the power and predominant implication of this latter system? What more probable usual stimulus can be supposed to the muscles of involuntary motion, or more natural occasional stimulus, through the medium of the brain and spinal marrow; or what more probable occasional stimulus to the voluntary movements, than the universally diffused agency exhibited in the chemical and hydraulic relations of the circulating system? We seem to find here, what we may elsewhere seek in vain, a frequent substitute for the irritations applied in the experiments from which these results are taken—a usual stimulus to one set, and an extraordinary morbid excitement to the other. Since spasm, an involuntary movement of voluntary muscles, cannot well be explained by the influence of the will acting successively on the origins of their nerves, though, physiologically speaking, in their

regular actions, this principle may direct the nervous influence to the required point, such morbid affection must arise from a foreign or unusual stimulus applied to the seat of excitement. Independently of organic derangements, what stimulus can be applied, or successively applied by a varying impulse, unless we find it in the effects of the circulating blood? or, how shall we better explain the occasional effects even of organic disease, than by the same process, which alone, perhaps, admits such a discontinuous affection of the nervous system. Under this explanation, we seem to understand those common affections of the involuntary muscles, the heart, stomach, lungs, which may result from menyngeal and other extended vascular fulness of the head, and similar affections of the other parts of the nervous system; and we discover in effusions and occasional pressure, and certain chemical relations, local effects which may induce spasm and the train of convulsive disorders.

That the usual disease, both as regards the voluntary and involuntary muscles, is an effect of blood in a morbid relation, seems more probable than any explanation taken from mere excess or defect of invisible nervous energy; and it is also more probable that remedial plans must have that former agency in view. If of nervous excess or deficiency in the system we practically know nothing; and if of various stimuli, which, according to experiments, may affect the muscles by irritation of the brain, the blood is universally present, always fluctuating, liable to varying determinations in force and extent, from

causes momentarily acting; if it be a stimulus, which, from experience, we can trace in its operations on many organs, inducing inflammatory and other changes in the cerebral and nervous as well as other structures; it does not appear why we should not still place a reliance upon this system, even under the admitted physiological facts derived from an improved acquaintance with the nervous constitution. Spasm and other similar affections are even now more easily explained by this intermediacy, than by any exclusive dependence upon a supposed nervous influence.

If the doctrine founded on most minute anatomical observation be correct, that every part of the body which is endowed with two or more powers is provided with a distinct nerve for each function; that, however intimately united in appearance, the origin of the spinal nerves and fifth pair is, in fact, composed of two packets of fibres, the posterior of which is destined for sensibility, the anterior for motion; a more minute and intractable source of affection is perhaps discovered, than was suspected in the experiments of the author lately quoted. The spasmodic affection would not, in this case, arise from an excitement of the whole origin of the nerve, but of a part of it; and, however difficult it may be to suppose such a separation possible, as that sensibility shall not be affected, while motion is so induced by its causes, and *vice versâ;* yet as this does really happen, and it is proved by the experiments that the sole stimulus is *not* the will; we must, in this case likewise, have frequent recourse to the sanguiferous

system, "the most considerable and constant of the "exciting powers," for the source and origin of morbid irritation.

Secretion is another physiological phenomenon, supposed to be connected with, or rather dependent on, the nervous system. The proof of this fact is taken from the circumstance, that the secretions are suspended by the division of certain nerves, and are restored by the communication of an influence supposed to be identical with their own. It is presumed, that, by a knowledge of these facts, you are enabled to discover the nature of some diseases dependent upon defective secretions at least, and to ascertain their most appropriate remedies; and that these which are, in the most complete sense, nervous diseases, and nervous remedies, bear no analogy to those which originate in the sanguiferous system. According to this doctrine, in a physiological view, the brain, spinal marrow, and ganglions, are the source of the nervous influence operative in the function of secretion. The nerves are the conductors. The capillaries are also mere conductors of the arterial blood, from which, by the operation of the nervous influence, the third product results. Of the mechanical or other agency of the vessels or stomach, following this union of the nervous and sanguineous influence, we are not informed. It may indeed be inquired, whether the stomach, as well as each other secretory organ, is a mere receiver or depository, upon the surface of which, from a combined influence, the separated fluid is formed; and whether, by a mixture of such

fluid with the ingesta, the digestive process occurs of the food, which is then to be propelled into the duodenum by the muscular efforts of the ventricular bag.

In looking at the proof of secretion, founded upon its absence under a division of the nerve, and its renewal by the galvanic intervention, some alternatives seem to arise to the proposed explanation: *first*, as regards the influence of the intercepted nerve; *secondly*, as regards the nature of the substituted influence, and the effect of other stimuli; *thirdly*, as to the real nature of the change effected.

(I.) An alternative of the immediate operation of the nervous influence in the production of the gastric fluid, may be found in the general change of relations, occasioned by a division of the nerve, and in the imperfect state of the stomach, the organ of secretion, under the injury done to its organic constitution; a state which might equally arise under the want of integrity, or diseased condition of any of its other component parts, as of that of the nervous portion. If, according to Mr. Bell, the fifth pair has a complete similarity in every respect to the spinal nerves, and, having a double origin, is at the same time a muscular nerve, regulating the voluntary motions of the body, and bestowing sensibility on its surfaces, we may see, in its division, many effects not exclusively allied to the conveyance of secreting power, and may find in it a variety of local function, which has been denied to the capillary vessels under their imagined analogy to the circulating system, and

under the supposition that no such variety can be consistent with similarity of structure.

In confirmation of such alternatives, it appears from the experiments of other physiologists,* that secretion has been known to proceed without the brain, and below an intercepted nerve. These facts, indeed, if established, are not conclusive, because the integrity of other nerves, and the substituted influence of other centres, may be assumed; and because, in the case before us, such influence seems to have been wholly wanting. The effects appear in this example to have been the consequence of the artificial influence alone which was introduced into the system, whether through the fifth pair, or the connected branches of other nerves.

It seems, however, a powerful objection to the doctrine which adopts the nervous influence as the usual origin and regulator of the secretions, that, according to Dr. W. Philip,† the nervous influence is subject to constant variations, which should, it appears, dangerously affect a process so necessary to health. On the same authority we learn, that the secreting viscera, the stomach, bowels, lungs, &c. are subject only to an occasional influence from the nervous system. Unless, therefore, there are distinct nerves for secretion, and for motion, the process must again be liable, in all ordinary circumstances, to variations which must seriously derange the local and general systems, and at all times render them subject to great irregularity of function.

* Pring on Nerv. Sys. p. 60, 62. † P. 273.

But, on these very accounts, it is, the doctrine of the same author, that secretion is, in fact, not dependent on nervous influence. "The relation of "the vessels of secretion to the nervous system is the "same," he declares, "as that of the heart and "vessels of circulation. Their function is inde- "pendent, but capable of being influenced. The "function of secretion requires a more regular supply "of fluids than could have been obtained, had the "usual action of the vessels depended on the nervous "system, which is subject to continual variation." It thus appears that the necessity for nervous influence, under common circumstances, is entirely removed; and that, just as in common circulation there are stimuli more appropriate to the heart and involuntary muscles, so there are other stimuli more uniformly acting, and more appropriate to the function of secretion, whether these be connected with the nervous, or with the vascular share in this process. We may, therefore, consider the case of interrupted influence by a division of the nerve as not really illustrative of the natural conditions, but as exhibiting, if any, an influence derived under circumstances not essential, and requiring an extra-ordinary agency for the renewal of the conditions which had thus been intercepted. The usual stimulus to the function or process of secretion is not the nervous influence. What is the usual stimulus? Is it the determination of blood?

(II.) It may, perhaps, be argued, secondly, that there is no proof of the nature of the effect produced

by the substituted galvanic influence, so as to warrant the assumption, that the nervous influence as such is necessary to the process of secretion. It is not yet proved that other stimuli might not equally produce the same effect. If, in health, the nerve supplies a stimulus by which the function may be affected, galvanism, or other stimuli, may be equally efficient, and the process of digestion, as far as secretion is concerned, may be carried on as well without the nervous influence as with it.

The explanation itself, which has been given, of the effects produced by the process of dividing the nerve does not seem strictly accurate. It is said, "the influence of the nervous system is capable of "its functions, after being made to leave the nerve, "and pass through certain conductors of galvanism."* Surely this is not the state of the case. It is not merely a connection and restoration of nervous energy through an inert conductor, but the introduction of a certain quantity of galvanic fluid itself, as would happen in all other galvanic processes, without the intervention of the nervous centre or structure. Has it ever been tried what effect would be produced, if the connection of the upper part of the nerve were omitted? or what would be the effect upon the contents of the stomach, if other stimulants besides the galvanic fluid were introduced into this viscus? Notwithstanding the difficulty which might attend this process, such a trial seems necessary, before any legitimate deduction can be made as to

* Journal of Science, vol. xiv. p 107.

the exclusive influence of the nervous energy in the process of secretion.

(III.) Thirdly, however, a question may still be entertained, whether the change effected in the state of the food by the substituted influence, be that which would have occurred if the natural process had not been interrupted; in other words, whether the process of digestion is really promoted by the galvanic influence. Some doubt, it appears, is expressed on this subject, even by those who witnessed the experiments, and admit the general facts.*

It is adduced as an additional instance of the disregard in which Dr. Parry held the nervous system, that he attributed the office of secretion entirely to the capillaries, "a system continued on "from the arteries, and ending partly in veins, and "partly, perhaps, in exhalants and secretory organs,† "and of which, certain branches, though often of "equal diameter with others carrying red globules, "not only in a state of health refuse to admit them, "but select, as it were, what parts of the blood to "receive, and what to effuse; and even combine its "several molecules, so as to form substances of new "and original properties."‡ It is, in answer, observed, that "this opinion is truly untenable, since it is "proved by the experiments of all modern phy- "siologists, (Mr. Brodie?) that secretion, more, "perhaps, than any other process, is under the

* Broughton, Journal of Science, vol. xi. p. 326.

† Elem. p. 81. ‡ P. 28.

"direct influence of the nerves."* Does this writer allude to the influence of the nervous structure? If not, how is Dr. Parry's doctrine incompatible with that even of Dr. Wilson Philip? He does not deny that a vital agency is communicated by the nerves, which must, however, be dependent for its perfection on the co-operation of certain other powers or processes, which may be resident in certain parts of the capillary system. Where, and by what instruments, is the product elaborated? The action of life in this process of secretion does not seem to be a mere reception of nervous influence in an unconnected and passive seat. The reception itself implies, indeed, another process, which is the nearest cause of the product, while the influence derived may, in fact, be only a stimulus, and if from the nervous centres, an extraordinary stimulus, exciting or calling forth the necessary action. Such a stimulus may be supplied by galvanism; but even, under this admission, we approach no nearer the act of secretion, the vital process which immediately occasions the result. This substitution, as it has been attempted to shew, neither proves an identity of the nervous and galvanic fluids, nor any more general principle of the vital action, which remains precisely as before, however this fact may extend its relations with other matter. All action is the result of a stimulus which excites motion; and, in motion, we see the operation of this new play of affinities. In other words, all stimuli act by affinity with the parts excited, and

* Edin. Med. and Surg. Journal, XV.

action is motion variously modified, according to peculiarities of structure. The life of the part combines variously with life presented to it, and the new combinations arising out of such motion assume a state of rest, till disturbed by other and stronger affinities. The supply of a stimulus is, however, perhaps, not so much the supply of power, as it is the means of calling forth power. But the action of life in the process of secretion is strictly connected with the parts in which the process itself takes place. The usual stimulus seems to be the same for all the organs of secretion, the great diversity to consist in a local predisposition or function, whether that have its seat in the capillary system, or in the nervous organic structure.

But to return to the original question. Where, in secretion, is the product elaborated? If, to use a gross analogy, it be only a chemical union, still separation is necessary ; and may not that process be as readily effected by a peculiar capillary office, as by the nerves, respecting which, as "carriers," or otherwise, we know so little? It has, indeed, been elsewhere shewn, from a presumed knowledge of the powers of the remainder of the vascular system, that the part of the series, which is destined merely to the circulation of the blood, could not be interested in the process of secretion. It has, however, no where been shewn, that the capillary share in such process may not be at least as probable as that of the nervous fibre. In the following argument, Dr. W. Philip has attempted to shew, that the vascular

system itself has no such concern, and his observations may be supposed in like manner to extend to the capillary vessels. "If the power of separating "and recombining the elementary parts of the blood "was bestowed on the extreme vessels by the influ-"ence of the nervous system, some change in the "distribution and motion of the blood in the web "must necessarily have been occasioned by interrup-"tion of this influence, under a slow destruction of "the brain and spinal marrow."*

It may be replied, that this argument is not conclusive, unless it be also shewn that secretion and the distribution and motion of the blood are the same; or, in other words, that those functions are analogous to that of secretion. It is not conclusive, if there are, as seems necessary, separate fibrils at least for motion, and for the secreting process. Neither is it a necessary consequence, if the vessels are, as we believe them to be, inactive towards the common function of circulation; and if they are, therefore, not influenced by the cause, which, under his supposition, should so affect them.

That there may be separate nerves for secretion and for motion, if the former process is dependent upon the nervous energy, and if the physiological facts as to the capacity of affection are true, seems, as has before been shewn, necessary, on the ground that, without such an arrangement, no regularity could occur in this important process. But the office of the capillaries is wholly unknown to us.

* Journal of Science and Art, p. 272.

They are no longer carriers. The usual vascular function has ceased with the termination of the necessity for circulation. It is, indeed, admitted by Dr. W. Philip, that they assist secretion by a mechanical effect. They act as a strainer, and separate the finer parts of the blood. Is this a mere mechanical and accidental separation, or may it not rather arise from a difference of vital function?

Before the objection of the first-mentioned author can invalidate the observations of Dr. Parry, he must prove to us where the peculiar agency resides, by which the gross separation is effected, and farther, in what structure the power is placed, which is the source of the difference existing in the various secreted matters belonging to the different parts of the animal body.

As it has been shewn in the Elements that "all "parts have peculiar functions which are justly attri- "butable to the different modifications of suscepti- "bility, derived from the specific powers or capacities "of the nerves, with which they are supplied,"* there does not seem any very evident reason why the specific function of secretion in seats should not have a relation to a specific office, locally assigned to that extreme arrangement of the circulating vascular system to which the term capillary is usually assigned.

Let us now inquire into the connection of the recent neurological discoveries with pathological and therapeutical indications, and see how far they are of use "in explaining the nature, and regulating

* P. 257.

"the treatment, of the deviation of the functions "of the animal body from the healthy state, and "particularly in the diseases whose symptoms are "most influenced by the mutual sympathy of the "vital organs."

The most important illustrations of the relation which such discoveries are supposed to bear to these purposes, are found in the circumstances connected with morbid states of the digestion, habitual asthma, and with other connected and sympathetic disorders, which have been already enumerated. Let us attempt to ascertain the extent and precision of the doctrines recently introduced, and to determine whether our knowledge of nervous diseases, as they are by distinction called, warrants, even in this day, a very exclusive attention, or the removal of the stigma with which the neurological systems of his day were marked by the Author of the Elements. These nervous doctrines are founded chiefly on certain principles which have been adopted by Dr. Wilson Philip, as the results of his experimental researches. They assume,

First, That secretion is effected by nervous influence.

Secondly, That irritation of the nerves induces nervous debility and its effects; among which is defective secretion.

Thirdly, That nervous irritation produces vascular debility.

Fourthly, That vascular irritation produces nervous debility.

Fifthly, That vascular irritation produces vascular debility, which is the condition of inflammation.

To the first of these principles allusion has already been made. Of the second, it may be observed, that there is no direct proof that, in the assumed cases, the effects are upon the nerves; or that this effect of debility, in any case, can, as is required, be admitted as an early and immediate consequence of irritation. With regard to the third, fourth, and fifth points, the experiments require frequent repetition, before they can either be established as entirely correct, or the supposed inferences be adopted as incontrovertible.

The morbid derangement of the function of digestion is divided by the author into three stages.

The first is said to be a mere nervous affection, and to consist of debility of the nerves from irritation.

In the second stage there is a continuance of nervous affection, which influences the capillary vessels, inducing debility of the vascular as well as of the nervous power, and thus constituting inflammatory affections. It is more particularly specified that a debility in the finer vessels of the pylorus is produced by the long continued irritation of its nerves, which themselves become debilitated, on the principle that whatever debilitates the muscular fibres, produces more or less, a similar affection of the nervous system.

In the third stage, sympathetic affections of various organs take place. A change of structure, or organic derangement induced by indigestion, occurs in the

abdomen, chest, or head. In these permanent diseases of distant parts, produced by sympathy with the state of the stomach, there is, first, either an affection of the nerves alone, or, secondly, of both nerves and vessels. The latter cases are, it appears, by far the most frequent.

In the first stage, which is said to be merely the consequence of irritation, upon what evidence is the assertion founded, that such irritation acts primarily on the nerves, when the contrary may be asserted with equal probability? Setting aside the inscrutable facts of predisposition, the first causes of indigestion are just as likely to act on the muscular structure of the stomach, or, according to these authors, on the analogous one of the vascular system, as on the nerves alone. Irritate the involuntary muscles by causes such as may occur in this condition, they are affected by such irritation, if not independently of, at least contemporarily with, any manifest changes in the nerves. In the case before us, it is evident that any reasoning deduced from the effects of irritation on the separate nervous structure of the brain, as occurs in our experiments, cannot safely apply to the connected structure of the stomach in the living and digesting animal.

But it may be said, there is evidence that secretion depends upon a function of the nerves, and that digestion depends upon a fluid secreted in the stomach. This may be true; but has indigestion no other causes? and is this solitary cause so certainly ascertained, and so accurately defined, as to warrant, in so complex an

operation, the assumed pre-eminence? How, in the living frame, are we to discover, when "the vessels are "healthy, and the power of secretion is alone in "fault," amid those complicated agencies and relations, which, in the exposition of Dr. W. Philip, occupy so large a space? or how shall we be able to discriminate the exact amount of participation, which the nervous system has in this derangement of the natural process? Are the state of the ingesta, the integrity and health of the vascular system, that of the secreting surfaces, and of other allied parts, so easily separated in the account of morbid affection? Admitting the nervous secreting influence to be in fault, is it clear that there can be no excess of this influence, and do we know the exact symptoms and consequences which might be connected with such a state?

But is it with more certainty established, from the effects of remedies which are said to be salutary in this stage of morbid derangement, that the nerves are the primary agents in the affair of indigestion? and do the curative means admit no alternative of explanation, which might be adopted in the absence of all evidence under the first head? Is it quite clear, that "we relieve the nerves alone from their load," by the treatment recommended; "by attention to "diet and exercise, and a proper use of aperient, "stimulant, and tonic medicines?" A reason, unconnected with this direct nervous interposition, is, indeed, given by the author himself of this doctrine, why in the headach, symptomatic of the two first stages, emetics and cathartics should be adminstered,

and blood-letting from the head should be adopted as the most effectual remedy ; they are said to "remove "the immediate cause of irritation, and occasion "depletion of the vessels of the head."*

It is not easy to discover the evidence by which, either in the way of primary affection, or of indirect influence by privation, the nervous system is maintained to be the part chiefly implicated, or to be the agent, by a reliance on which the treatment of diseases may, in any manner, be more safely conducted, than upon our former plans. It will not, it is feared, prove a very satisfactory statement, if, on one side, we place the rare and uncertain means which are adopted on the theory of a direct and indispensable nervous implication, and, on the other, the list of usual curative means which are wholly unconnected with such a reference. If there be a deficiency of nervous power, and we can, by any means, ascertain such a want, the galvanic influence may be of use; but it will be recollected, that even this cannot be suggested by the experiments which have been made, or by the consequent theory which has been adopted. In these experiments, a total interruption of the nervous influence was effected, which does not happen in the living body ; and no regular inference can be drawn with regard to the possible effects, in cases which bear no such analogy. We must here, as in other instances, confide in our experience alone.

It appears, that the data, by which the nervous connection in the pathology and symptoms of this

* On Indigestion, p. 282.

disease is established, are very insufficient, and that in our therapeutical considerations, we can place little reliance upon the nervous theories. Admitting the importance of the alleged discovery, we can but set it down as one among many other means of more palpable application, and, perhaps, of as beneficial effect. Although it were true that the want of digestion were connected with deficient nervous energy as a stimulus, we neither derive from such knowledge a certain general rule of practice, nor the power of adapting our treatment to the particular circumstances of the case. Here, as well as elsewhere, we must have recourse to visible agencies, or to experience ; and while we trust to causation of a more evident kind, must still rely upon more manageable means of relief.

Various sympathetic diseases, arising under a state of indigestion, are, for this reason, supposed to have a very intimate connection with immediate derangements of the nervous system. Is there any better evidence as to their nature in this connection, than as to that of their primary disease ? Is it clear, that, however much in them, no less than in all other disorders, the nervous system must participate, it is otherwise than secondarily affected ? Among the affections designated as sympathetic, and as dependent on the nervous system, nervous headach, pure nervous apoplexy, and habitual asthma, have already been mentioned. It has been shewn, that the headach in the first stage is, on very unsatisfactory grounds, explained as a mere affection of the nervous system ;

and that the proposed treatment admits an alternative, which precludes the necessity of the adopted inference. The same affection in the second stage is admitted to be of an inflammatory nature.† Of the value of nervous medicines in this complaint, we have the testimony of the author himself, who says, "their "effect in relieving the headach of the first stage is "often considerable, but very uncertain."* It is, indeed, said, that, alarming as are some of the inflammatory affections of the head, its nervous affections connected with irritation of the digestive organs are often still more so. The history of nervous apoplexy which follows, is, however, full of assumptions which do not immediately claim our assent. Possible as may be the existence of such a disease, there is in the account of it neither evidence of prior independence of the sanguiferous system, nor of subsequent entire connection with that of the nerves.

These nervous affections appear to be of two kinds, the one arising from long continued irritation of the nerves of so important a part of the system as the digestive organs, directly debilitating, and sometimes wholly destroying, the source of nervous influence; the other, from the irritation affecting the state of the vessels of the brain, and consequently the distribution of that portion of the blood which is sent to the head.‡ Our present concern is with the first. Of its existence and nature we have no evidence, except in effects which are similar, either under the common alternative of sanguineous apoplexy, of that

* On Indigestion, p. 282. † P. 287. ‡ P. 289.

modification which exists without extravasation, or permanent fulness of the vessels; those other fatal cases of epilepsy or mania, which indicate no change in the brain, or of those instances of death, which, as under syncope and diminished action of the heart, inducing vertigo, actually arise from deficient circulation in the head.*

"After more severe attacks of giddiness, temporary "loss of power, &c., &c., than usual, and sometimes "without warning, the patient, it is said, suddenly "falls down, and in some few cases almost immediately "expires." This disease, "in contradiction to "apoplexy, arising chiefly from the state of the vessels, "is properly termed nervous apoplexy, the most fatal "of all its forms;" and it has been remarked, that, in *some* cases, no morbid appearance presents itself on dissection. The fatal derangement is in the nervous system alone, whose structure is too minute for our observation. If the usual plan of bleeding in all cases of sudden insensibility be here resorted to, the disease is only the more suddenly fatal. It is presumed, that "a succession of slight causes has "gradually changed the mechanism of the brain," a fact of which there is not the slightest evidence. This "most fatal form of apoplexy," which only in some few cases immediately destroys the patient, is, however, necessarily an extremely rare disease; because it very seldom happens that the causes continue long enough so to derange the finer mechanism of the brain, as to produce loss of function, without influ-

* Elem. pp. 230, 260, 281, 287.

encing the state of the circulation in it in such a manner as to produce a fatal effect in this way.* Such is the history of this invisible and most rare disease, with which, from the few cases in which it is immediately fatal, an accurate acquaintance after death must be a still more difficult and infrequent occurrence. It may be safely left to any unprejudiced person to say, how far, in these instances, any reliance can be placed on this nervous theory, to the exclusion of other pathological doctrines, or how far it may be considered a safe foundation for medical treatment.

"Permanent Dyspnœa" is another disease which is a frequent attendant on the process of indigestion. It seems to be wholly a nervous affection, and to be often the effect of repeated attacks of inflammatory dyspnœa.† But what are the evidences, that, in sympathetic affection of the lungs, and in the derangement thus named, "there is a permanent debility in the nervous "system of the lungs, with little or no affection of the "sanguiferous system remaining." There is no such evidence from dissection. The only proof is derived from the theoretical supposition, that inflammatory action induces "debility of the nerves," and can be suspended by such a state. But galvanism, while it relieves this affection, is not serviceable in the asthma, which returns in violent paroxysms, and is supposed to arise from "spasms." Setting aside this reference to a condition as little connected with observation as the nervous theory itself, is there no possible differ-

* On Indig. p. 291. † P. 279.

ence in the degree of morbid affection, in which the same remedy might not produce equally beneficial effects? Is it evident, that galvanism acts in a manner different from that in which other remedies, having no evident connection with the nervous system, act? Is it a dispenser of nervous power,or a stimulus, as in its application to the muscular system? Has it the supposed specific influence of galvanism in an increase of the secretions, an effect which no other power can produce? In this case there might, indeed, be only a difference of more or less, and the influence as to this form of asthma being wholly dependent upon deficient nervous energy, might still not be strictly admissible.

If, as appears from Dr. Wilson Philip's view, all the diseases, which are, strictly speaking, nervous, are apt to run into febrile disease,* and febrile disease necessarily produces debility of the nerves ;† if there is no sensible evidence connected with nervous states by which we may strictly define them ; and if we have sufficient evidence of the existence of febrile states from their sensible phenomena ; it does not appear why, in the wide circle of relations constituting disease, we should not prefer those circumstances which are demonstrable, to others which are unknown and obscure. There seems no reason why we should not chiefly rely upon the facts of the sanguiferous system, and still renounce those mysterious and latent properties which are involved in all the doctrines having the nervous system for their basis.

* P. 36. † P. 119.

The doctrine of nervous debility, in its application to practice, gives us no information as to a prime constituent of human knowledge, the order of phenomena. It neither teaches us the structure nor function which is first implicated; nor, from the essential nature of the case, can it furnish us with any instruction as to the real quality of the diseased conditions.

The anatomy of the nervous system is becoming gradually developed by the industry of many intelligent and indefatigable men, and will, no doubt, receive increment and improvement, in proportion to the zeal and talent thus employed.

The recent physiological doctrines, which respect the affection of the muscular fibre by nervous influence, and the dependence of the process of secretion on the integrity of the nervous structure, are of the greatest interest, and are also supported by very able men. They have their basis in experiments of a gross kind, which will require frequent repetition before they can be adopted as incontrovertible facts, or justify the important inferences which have been deduced from them.

The neurological systems of pathology are still eminently defective, and the therapeutical indications are derived from uncertain and intractable principles. "Our safest practice, even in the present day, is that "which best suits our faculties." Put together all the cases which have been strictly called nervous, in which, under an admission of the required data, medical practice is supposed to have improved, or

which are supposed to supersede that exclusive attention to the sanguiferous system, which has, in ignorance, been charged upon its advocates, do they bear any proportion to those which may be arranged under other heads? or, rather, do they not form a very insignificant catalogue, in comparison of those which are palpably connected with derangements of this important part of the animal economy; or require, for their treatment, the consideration of other related agencies?

Galvanism is a valuable remedy, upon whatever theory its use may be adopted. In one or two cases it appears to afford relief, and it may do the same in one or two instances of supposed analogy. Is it as distinct in the principle upon which it is administered, as intelligible in its process, as certainly efficacious, as former remedies? It is said, that a temporary, or even a more lasting, effect may be produced. Is it a remedy where all others fail, and will this alone perform a cure? We hail it, then, and hope it may be found so universally serviceable in its application, as to warrant the praises which have been bestowed upon it; and that the principle which regulates its employment, may be extended with equal advantage to many other cases, whether of an analogous or a different nature. If, however, this means is found to be "more successful than any other, in obstinate "general nervous debility, in certain species of fever, "and to be a valuable remedy in cases attended with "deficient nervous energy;" we must first learn the exact symptoms and indications of these mysterious

states, and then may certainly have recourse to it, with an expectation of the promised benefit and success.

With regard to the diseases, which, by Dr. Parry, have been distinctly specified as belonging to the nervous constitution, if, by accurate research and experiments, we have advanced a step in explanation, do we, in any wise, alter the fact, that the condition of a heart easily excited, and all its consequences, arise from the influence of the vascular system in the brain? that, in an early stage of disease, induced by a thousand causes, and arising from predispositions natural or acquired, such a state of excitement may be kept up, as by occasioning these irregularities in the action of the heart or vessels, will proceed still farther to produce by reaction the various symptoms, called, by distinction, nervous; which, even upon the nervous theory of spasmodic action, resulting from successive excitement of the origins of nerves, can best be explained and treated on the doctrine of positive or relative momentum? How, under any of the new doctrines, do we get rid of the opinion, that "in the "greater number of maladies," this condition is not only a concurrent cause of disease, but is, as has before been observed, the sole ostensible guide and criterion of practice.

Is it not admitted by the authors themselves, who have with the greatest care investigated these matters, that the cause of many of the nervous affections is foreign to the structure itself? Is it not "inferred, "that the blood-vessels have a principal share in the "production of painful affections of nerves, from

"their increased size in nerves, that have been "affected with much pain;"* that "opisthotonos, "convulsions, and death, have arisen from a thickened "and opaque state of the tunica arachnoidea "about the first dorsal vertebra;" that "after "epilepsy and erysipelas, the whole tunica arachnoidea "had a very opaque and thickened and granulated "appearance, as of previous inflammatory action?"— while, in all these instances, the state of the parts with which it was connected, was such as to escape observation. In speaking of paraplegia, it is a remark of the same author, that "the diseases of the nervous "system are so obscure, that he dare hardly hazard "a conjecture respecting the cause of the symptoms; "while similar cases led him to suppose a great "determination of blood to the medulla spinalis." These and many similar results will be found, openly or by implication, acknowledged by the authors, who are themselves most devoted to the theories of nervous atony, and deficient energy.

Whatever advancement may since have been made, there was certainly nothing known, or practised, when Dr. Parry wrote on the subject of nervous diseases, which could authorise the adoption of such knowledge or treatment as a principle, or rule of conduct, and which, from its injurious tendencies, did not justify him in excluding, then, and for ever, "the neurological systems hitherto extant." That this author is not understood, and that the nature of nervous affections themselves, in the view, which,

* Swan's Obs. on the Nervous System, p. 43.

after long and cautious experience he had taken of them, is mistaken, is shewn by the opinion which Dr. Wilson Philip expresses, as to the state essentially implied by the nervous constitution. Of the nervous apoplexy, he says, it is most apt to supervene "in exhausted states of the constitution, or in what "are called very nervous habits." Now it may be maintained, that though, from the operation of different causes, similar effects may follow in one and in the other state, very nervous habits are not necessarily connected with exhausted states of the constitution, and that the symptoms eminently and decidedly nervous occur, on the contrary, in young and otherwise healthy subjects. Such are all the hysterical, convulsive, and spasmodic states as they occurred in the original case described by Dr. Parry, in which, in fact, every variety of nervous affection was assembled. Such are the symptoms which we daily see in the higher, more rarely in the middle, and most rarely of all, in the lower clases of society,—notwithstanding the exhaustion of protracted and ill-regulated disease, bad living, and irregular habits. The predisposition to these varieties of nervous affection is generally, in fact, found in the young, and otherwise vigorous, and is often independent of exhaustion, or extraordinary excitement. No more complete instance of nervous affection, in all its degrees, can be exhibited, than is at present witnessed by the author of these remarks in a young female of fortune, of healthy and florid appearance, of habits temperate, of mental feelings unsophisticated, and uninjured by any

demoralization from ill-conducted education or habits; in whom high susceptibility of impressions, tremors, palpitations, swimming, fancied or real fallings down, occur, with a pulse generally regular, an appetite sufficiently good, and a cheerful temper. It may not be devoid of interest to mention, in this place, also, that the original case of nervous affection, which gave rise to the observations of Dr. Parry so long ago as the year 1785, still continues to furnish abundant evidence of the accuracy of his conclusions. Notwithstanding the period to which, even under this highly nervous predisposition, a comfortable state of health has, by care and management, been enjoyed, occasional relapses, of the greatest possible degree of violence, take place. The most powerful convulsive efforts under these attacks are extended, almost without intermission, through several successive weeks. Every muscle in the body is thrown into the most extraordinary contortions. Hiccup, and spasmodic suppressions of the breath, occur almost to suffocation. Under these circumstances bleeding affords a temporary relief, and compression of the carotids has, in repeated instances, at once suspended all the violence of these fearful paroxysms, which immediately recur on the removal of the compressing agent. Nature's remedy in this case is a very copious hæmorrhage from the nose, which is frequently continued, almost uninterruptedly, for seven or eight hours. In the interval of freedom from these symptoms, the patient has the characters of health, and is capable of much exertion.

It may be possible to extend to many other morbid affections, besides those which have been formerly enumerated, the supposed insulated or connected influence of the nervous system in disease, and to exhibit, what the Author of the Elements has never denied, the operation of a number of causes acting, in all appearance, directly on that constitution, and not through the medium of the circulation. We may, with Whytt, consider few diseases as not nervous; and may, with others, ascribe this origin to cholera morbus, inflammation, epilepsy, and various other maladies. We may be told that "sudden "bursts of grief, the electric fluid, carbonic acid, the "carburetted hydrogen, and, perhaps, others of the "deleterious gases, and some animal and vegetable "poisons, are capable of instantly destroying the "nervous influence, and thus producing death." That such causes produce instant death, we are well aware; but we must have far better evidence as to the mode in which, and of the properties on which, they act, before we can adopt this premature conclusion. When the mode in which they act is finally settled, it may then be time to inquire, whether Dr. Parry excluded them; or whether he did not rather come to the same conclusion with the author of these suggestions, when he remarks, "that the "total death of the brain may arise from causes "which do not act through excessive impetus of "blood;" "that the effects of certain gases, and "other poisons, disorder the intellectual faculties by "some operation different from mere sanguineous

"impulse;" and that the morbid changes, which he describes, are not intended to comprehend the maladies which arise from the influence of mechanical causes, or "of infectious or other miasmata."*

In proof of the early and particular attention which Dr. Parry bestowed on the nervous system, it may be interesting to refer to the following, from among many Experiments, which were proposed by him in the year 1776. It will not be forgotten, how long prior to the galvanic or other animal electric discoveries, these suggestions were made, and it will be seen how nearly, at that early period, he approached those facts, which have since added lustre to the name of their author, and developed the connection of a most important agent in the economy of the animal system.

Exp. 10. To lay bare the sciatic nerve in two places, and to cut it through in that part most distant from the brain, which is laid bare. Then placing the two ends of the nerve, so as to be opposed to each other, without, and in other experiments with, touching, to observe whether an electrical spark or shock, applied to the part of the nerve laid bare above, produces any motion in the limb below the part divided.

Exp. 11. To compress a nerve with silk, wax, and other non-conductors, and treat the animal as mentioned above, except cutting the nerve.

Exp. 12. To observe whether an animal is killed sooner by giving a shock to a nerve laid bare, than to

* Elements, pp. 275, 277, 320.

muscular flesh, &c. at random, at the same distance from the brain.

Exp. 13. To tie a nerve with thread, and compress it with conducting substances, and treat the animal as in experiments 10 and 11.

Exp. 22. To cut through the sciatic or some other nerve, in a living animal, take out a part, and make a communication between the two ends, by means of some substance which is a conductor. Then to observe whether the animal, before void of motion and sensation in the lower extremity, can be capable of either, when the two ends are thus brought to communicate.

Exp. 23. To give shocks and sparks to an animal in the same circumstances, and observe the effects of these on the lower extremity; and whether, during the giving of the sparks, the animal is at all sensible of the action of a stimulus upon the lower extremity.

Exp. 25. To cut the sciatic and anterior crural nerves of a dog, and observe how long it is before he acquires sensation. If he do not in a long time, to kill him, and observe whether the nerves be regenerated and united. [1776.]

DESCRIPTION OF THE FIGURES. (PLATE.)

Fig. 1. Vessels in the web of a frog's foot, *d* and *e*, are two venous trunks connected by the branches *a*, *b*, *c*, &c. No extraordinary pressure or stimulus was employed. From *a* to *b*, the blood never flowed uniformly, but sometimes ran into *e*, at others into *d*, through *c*. The varieties, whether of the retrograde or oscillatory motion, always depended on the force and rapidity of the currents in *e* or *d*. Upon some occasions there was a double stream in *a*, to *b*, one part of which flowed towards *e*, and the other towards *d*, according to the violence with which the fluid was impelled from opposite sides. These double and opposing streams, in the same vessel, are divided by a dotted line, and one of them is coloured red.

Fig. 2. By elevating or lowering the web, by pressing or removing pressure, the fluid in *c*, could be made to flow into *a* or *b*, for an indefinite length of time. Again, according to the prevalence of the current in *a* or *b*, the blood in *c* moved into one or the other, and oscillated entirely under the influence of these currents.

Fig. 3. In consequence of obstructions at *d*, *e*, and *f*, the blood in *a*, *b*, an arterial branch, could not circulate in its natural direction; the oscillatory movements in its stream were entirely regulated by a rapid arterial current at *e*.

Fig. 4. In the vein *c*, *d*, the blood moved up or down, according to the supply of blood, and the velocity in the veins *a*, *b*. In this instance, the effects of gravitation, in producing the retrograde and oscillatory motions, were very conspicuous. By an almost imperceptible change of position, the blood might be poured either way. By the same process, the capillary circulation was, at times, entirely suspended.

Fig. 5. In the upper part of a vein, the blood stagnated, and was of a bright red colour. Into the same vein ran a collateral branch, pouring in a much darker blood, with great rapidity, so that one half the vessel contained stagnated, and the other half, circulating blood. From *a* to *b* the stream was very rapid.

APPENDIX.

APPENDIX.

THE following extracts from Letters addressed to Dr. Parry, at an early period of his medical inquiries, shew the opinion which was at that time entertained, with regard to their value and originality, by physicians of eminence, as well in England as in America. It will not be difficult to establish his claims on these points, and in relation to many other connected subjects, by a selection from an extensive correspondence, maintained during forty years, with many of the most distinguished practitioners in Great-Britain and other countries.

"DEAR SIR,

"Permit me to thank you for the unex-
"pected pleasure of your letter, and particularly for
"the valuable facts it contains. Your treatment of
"Miss B. does you great credit, for her constitution
"is not happily framed for making converts to
"physic. - - - - - - - -

"This brings me to the essential part of your letter, "and a very essential one it is. An improved mode "of treatment in the disorders called nervous was "certainly much wanted; and judging from my own "observations, I may congratulate you on having "effected it, though I do not in the least pretend to "have anticipated you in your improvements. After "I began to think for myself, I soon grew tired of "the usual modes of practice in these cases. I "returned back to the path chalked out by Syden- "ham, and used *copious* evacuations of blood at first. "Things succeeded better, but not as I wished; and "it remained for you to point out the cause of the "failure. I considered these cases, as I did that of "Mrs. P., as dependent upon morbid determination, "or rather congestion, and yet I never was struck "with the effects to be expected from *topical* bleed- "ings. You, Sir, have removed this mist, which, "as it now appears, so unaccountably retarded my "progress; and I have already put your ideas in "practice in more than half a dozen cases, with "every advantage you have taught me to expect. "In maniacal cases, I have seen the happiest effects "from cupping on the head, but still I never thought "of treating the more common nervous disorders in "the same way. I have cured some cases of insanity "by digitalis, cases which I thought dependent on "effusion; but your observations make it probable "that the sedative effects of the medicine on the "circulating system might have a considerable share "in the cure. I shall think myself honoured by

"your future communications, and remain, Sir, with "great respect,

"Your obliged and obedient servant,

"WILLIAM WITHERING.

"*Birmingham, Oct.* 29, 1788."

"DEAR SIR,

"Dr. R. delivered to me your very agree- "able favour of the 5th July last. Previously to its "receipt I had read your observations in their en- "larged state, in the London Medical Memoirs, "vol. iii., and hope, by the return of the present "conveyance in the Autumn, to forward you a copy "of an American edition of them. - - - -

"Your theory of nervous diseases is very nearly "similar to one which has been taught by Dr. Rush, "in his Lectures, for 3 or 4 years past; and he "derived much satisfaction when I informed him of "the coincidence of your opinions. Ever since he "has adopted the theory of the influence of the "arterial system in creating diseases of the nerves, "he has been uncommonly successful in the treat- "ment of epilepsy and other similar complaints, "supposed to depend upon an asthenic state of the "system, by gradual depletion; and this too after "they have not only resisted, but been rendered "worse by, the continued exhibition of powerful "tonics. In my own practice several cases have

"occurred in proof of the success of debilitating "remedies in nervous complaints, which have long "since satisfied me with regard to the truth of the "principles on which I acted. - - -

"The influence of prejudice among our medical "practitioners, and their attachment to first received "impressions, has, as yet, prevented the practice "from being generally adopted. The dread of "innovation, however, and of the triumph of truth "over error, when the latter is connected with long "received opinions, which appears to subsist among "many of our medical brethren in this country, in "as great a degree as that of a (medical) reform in "Europe does with many there, is gradually wearing "away, and will finally vanish in proportion to the "establishment of the empire of reason. I have had "the happiness to see, that, notwithstanding much "opposition, by theoretical arguments, to the progress "of a mode of treatment proved by both public and "private practice, it has gained very considerable "ground, and will in a short time, I am in hopes, be "finally established - - - - -

"With the sincerest wishes for your prosperity "and welfare, I remain

"Your very affectionate friend,

"JAMES MEASE.

"*Philadelphia, July* 10, 1794."

It will be remembered that Dr. Parry's Essay was written in 1786—7, and published in 1788.

The insertion of the following additional extracts may be permitted as conveying the sentiments of men distinguished by their knowledge, liberality, and extensive medical practice.

" SIR,

" The subject of our conversation the other " day, at Bath, has frequently occupied my mind " both before and since. The line of my profession " imposes the consideration of it as a duty, and I am " particularly anxious to collect any rays of light to " throw on a department of medicine more dark and " beclouded than any other. Patients labouring under " mental alienation are too frequently consigned to a " class of men, who are more interested in the conti- " nuance of their indisposition, than in their recovery; " and here a principal source of information, that might " be very productive, is cut off. Books only furnish " detailed cases, visionary reasoning, practice empy- " rical, and very little of the rationale. Even in the " most friendly discussions among medical men, this is " a topic uniformly shunned; and I must own, that " from our ignorance of the connection between the " mind and body, and their reciprocal influence, the " prospect of our ever being able to arrive at any " degree of certainty in the treatment of mental " disorders is not flattering. Under this consi- " deration, I could not but be gratified with your " ingenious ideas, and pleased to find a man who " will not submit to follow the beaten track. Theory

"and practice, experience and facts, conspire in proof "of your position, that in most cases of mental "derangement, there is determination to the head. "That other causes may exist, must be granted; such "are mechanical causes, exostoses, malconformation "of the cranium, or vessels of the encephalon, &c., "which must be deemed hopeless, and beyond the "reach of human exertions. But not to take up "more of your valuable time, my motive for writing "was to say, that if you had projected any experi- "ments to ascertain the truth of your hypothesis, "which my line of the profession might afford a good "opportunity of making, and to the management of "which I was equal, I shall be happy to second your "wishes with my best abilities; and promise the most "sedulous attention and faithful report. The "management of any plan for the relief of suffering "humanity, under your direction, will afford parti- "cular pleasure to

"Your most obedient,

"JOSEPH MASON COX.

"*Fishponds, Dec.* 15, 1797."

The following letter from a very skilful and experienced Physician in London, who followed the methods of treatment, which were laid down by Dr. P. alone, and which are now so generally received, shews at how late a period they met the decided

opposition, not only of many persons of rank and character, but of several of the most eminent and fashionable amongst the practitioners of the metropolis.

"My Dear Sir,

"I have not seen Miss C. for some time. "She had two dreadful attacks, one at the end of "July, the other about the latter part of August. "I knew of no means of relieving her, than by bleed- "ing till the symptoms of pressure were removed; "and, in following the dictates of conscience and "experience, I certainly endangered my reputation "as a physician. But this has been more than once "discussed between us, and we have always been of "opinion, that the discharge of duty was paramount "to all personal considerations. The practice, "though it saved her life, and was in truth the only "method by which it could, under such circumstances, "have been saved, raised a great clamour against me "from many of her friends. Of this, all I can say "is, that I must, in similar circumstances, always act "in the same manner, whenever it shall be my "misfortune to have the care of a patient labouring "under this complaint. My opinion, which I gave "freely to her parents, was, that although the attacks "were eminently dangerous, yet my experience of "the disease induced me to think that the necessity "for bleeding would cease, and that her health "would most probably be restored. Since I have seen "her, which is now three weeks, I find Dr. ——

"has seen her several times in consultation with "Dr. ———. What plan they have advised, I "know not, &c. - - - - - -

"Your very affectionate and sincere friend,

"* * * *

"*Sept.* 17, 1802."

This young lady's treatment was altered, and within a very few years afterwards, under other measures, she died of apoplexy. See her case and dissection.

"DEAR SIR,

"I saw Mrs. T. yesterday evening. - - "I made the experiment which you had already "tried. Upon compressing the carotid artery of the "right side firmly, the involuntary motions of the "fore-arm, arm, &c., were in a great measure checked, "but were not at all affected by a similar pressure "upon the left carotid. This is certainly a strong "proof of the truth of your opinion in this particular "case. Many of the cases which are generally "called nervous do, I believe, depend upon too "much blood being carried to the head, or with too "much momentum. I have, however, known "instances where no relief had been received from "*very copious* and *repeated* bleedings, and yet the "complaint was subdued by soothing medicines, or "by medicines of a strengthening nature. It is

"always *safest* and *best* to proceed upon the first "idea; and if no advantage is gained, then the other "method may be tried. I have accordingly directed "Mrs. T. to take two grs. of extract of hyoscyamus, "twice a day, and a cooling medicine every evening. "Mrs. Baillie joins me in best compliments to you, "and I remain, Dear Sir,

"Your very faithful servant,

M. BAILLIE.

"*Lower Grosvenor-street, Feb.* 1, 1805."

"DEAR SIR,

"I felt greatly obliged by your letter "respecting General S———. I am happy to say, "that his health is restored in a degree far beyond my "expectations, when he left London for Bath, and "which I cannot doubt is to be ascribed wholly to "the judicious plan of treatment you adopted. It "was one of those cases, (dropsy,) that under the "ordinary stimulant plan, with the large employment "of mercury, would, I have no doubt, have been "lost; whereas by considering it as originating in "inflammation, and applying to it the antiphlogistic "method of cure, its progress was fundamentally "arrested, and health restored, as far as the disorga-"nization produced by the disease admitted. I have "derived much satisfaction from the result of this "case, as it confirms strongly the opinion I have

"long entertained, that the majority of such diseases "originate in inflammation, and can only be success- "fully combated by keeping this idea constantly in "view, and acting on it with such qualifications as "the existing state of things may require, or the "general health allow.

"I have advised Gen. S. to adhere strictly to the "plan you laid down for him. It is his wish that I "should again communicate with you on the subject, "and it will give me much pleasure to hear from "you at your leisure.

"I remain, &c. &c.

"H. CLUTTERBUCK.

"*New Bridge-street, June* 10, 1812."

For the records of a treatment on similar views, adopted as early as the year 1784, see cases of dropsy, "Collections."

Amongst the connected subjects to which allusion has been made, and in the consideration of which, it might be easy to establish for Dr. P. the merit of a different mode of treatment from that which was in general use, may safely be mentioned Acute Rheumatism. The practice founded on the usual theories of this disease was, in the recollection of many very young practitioners, entirely opposed to the method now commonly adopted: and the employment of

cool air and regimen, instead of an accumulated heat, for the purpose of procuring profuse perspiration, was by Dr. P. introduced and recommended, through many discouraging obstacles, and at the frequent risk of the loss of credit and professional emolument.

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